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Piaget Interprets Himself

Jean Piaget

Les Mécanismes Perceptifs: Modèles Probabilistes, Analyse Génetique, Relations avec L'intelligence. Paris: Presses Universitaries de France, 1961. Pp. ii + 457. NF 28.

Reviewed by J. RUTSCHMANN

Jean Piaget is professor of psychology at the University of Geneva and at the Sorbonne. At Geneva, he is Director of (1) the Psychology Division of the Institute des Sciences de l'Education (2) the Laboratory of Psychology and (3) of the Centre International de'Épistémolgie génetique. He also edits the Archives de Psychologie. *His most recent books, both published in English, are* The Psychology of Intelligence *(1956) and* The Growth of Logical Thinking from Childhood to Adolescence *(1958). J. Rutschmann, the reviewer, is a former student of and collaborator with Piaget. He is now Associate Research Scientist at the New York State Psychiatric Institute and, at the same time, is lecturer in psychology ana research associate in psychiatry at Columbia University. He continues a research interest in sensory and perceptual processes and, in addition concerns himself with quantitative and psychophysiological testing procedures.*

THIS INTERESTING and stimulating book, which will give the reader a better knowledge of some of Piaget's recent work, is essentially a synthesis of 38 articles published by Piaget and his associates and students since 1942 (in the *Archives de Psychologie)* and of about 15 new articles concerned with developmental aspects of perception. Piaget states that his reasons for presenting the present synthesis are twofold: (1) to facilitate the comparison and confrontation of published data scattered over 19 years of a periodical; (2) to present a more systematic and more uniform theoretical treatment of these data.

In line with his permanent epistemological interests, Piaget proposes to study perception in order to elucidate the following major problems:

a) Does concept-formation ("intelligence") proceed by abstraction from perceptions, or are perceptions (or perceptual products) merely utilized in more complex cognitive systems which, in turn, may modify (i.e., complete or correct) the perceptions? Which are the similarities and the differences between cognition and perception?

b) What is the epistemological value of perception as compared to other forms of knowledge?

The first of these problems is clearly a psychological issue concerned, among other things, with the perennial controversy on nativism vs empiricism. The second problem, by contrast, is usually considered non-psychological in nature, since agreement on a measure of epistemological value is essentially lacking.

Because of limited space this review is confined to some of the psychological issues of Piaget's book.

Piaget actually formulates a theory of perception applicable to plane optico-geometrical illusions and proceeds to accumulate evidence in support of his theory. He also examines some problems dealing with constancy and with the perception of movement, of velocity and of time. These are perceptual phenomena whose theoretical treatment can hardly be expected to be more than illustrative or to represent more than indirect support of the theory, either by analogy or by demonstrating to the reader that some relational analysis of complex phenomena is possible.

LIKE BINET, Piaget distinguishes two types of illusions: *"primary" illusions* or illusions yielding systematic errors which decrease with age, and *"secondary" illusions* or illusions which yield systematic errors which increase with age (or which do not decrease with age).

Primary optico-geometrical illusions: Piaget states that the systematic errors in primary illusions (illusion of the rectangle, of the L-figures, of the angles) vary systematically as a function of spatial or geometrical parameters of the figure (size, proportions, angles, number of cross-lines, etc.), and that the maximum positive, maximum negative and zero systematic errors occur for the same parameters of the figure in-

dependently of age. However, the magnitude of the systematic error (when different from zero) is a function of age, i.e., it decreases with age.

Piaget has formulated the "law of relative centrations" to account for the changes in systematic errors as a function of the parameters of the figure; however, the function P determined by the law only indicates for which parameters of the figure the positive and negative maxima of the systematic error occur, and for which parameters the systematic error vanishes, i.e., the theoretical function P does not quantitatively predict the systematic errors. The function for P is most frequently a quadratic equation, its terms are sums, ratios and products of lengths representing actual (i.e., presented) or virtual (i.e., not presented) lines of the illusion figure. Typically, one or a few parameters of an illusion-figure were varied in Piaget's experiments, and the empirical functions relating the systematic error to the independent variable are compared to the theoretically derived functions, the implication being that a satisfactory "qualitative" fit of the empirical and theoretical function would support the theory. This approach poses three types of preliminary problems:

1.) The psychophysical measurements of the perceptual deformations were most frequently made with illusion-figures in which one of the comparison-elements measured 50 millimeters, the usual test-situation involving binocular viewing of the figures drawn in india-ink on non-gloss white cardboard. Variables such as luminance, contrast and visual angle were usually neglected or not systematically varied, although Piaget concedes that they may have an effect. This neglect, which is commonly seen in many recent publications in the area of perception, makes it difficult for the reader to evaluate a theory which deals with relatively small effects (expressed in per cent of the standard, most effects are smaller than 15%), especially since data on relevant difference thresholds are not given. Thus, for example, the question of whether or to what extent time can be traded for intensity in tachistoscopic experiments without changing the systematic error is not dealt with. Consequently, the connections between Piaget's theory of perception and psychophysiological and psychophysical theories remain uncertain.

JEAN PIAGET
Cliché P.U.F.

2.) The law of relative centrations does not predict exactly which real or virtual elements of an illusion figure have to be substituted for the various terms of the theoretical function. In other words, the empirical data may, at least in part, have been utilized for each particular application of the law.

3.) The method for deciding whether a particular theoretical function "qualitatively" fits the empirical data is not specified. This situation is rendered more problematical by the particular qualitative relation which, according to Piaget, maintains between the theoretical and empirical functions: the positive and negative maxima of the theoretical function correspond to the positive and negative maxima of the empirical function, whereas the zero point of the theoretical function corresponds to the zero systematic error determined empirically. In other words, the inflection points of the theoretical function are not used, and the criteria for qualitative correspondency appear as heterogeneous.

Piaget has developed a probabilistic interpretation of the law of relative centrations, the probabilistic model being based on the idea that some process is needed to produce error, and another process is needed to produce compensation or attenuation of errors (at least in primary illusions). Piaget postulates two mechanisms. The first mechanism is a probabilistic sampling (termed *"rencontres"*) of elements of a line as a function of time. The absolute subjective length of the line is a function of the number of elements of the line that have been sampled prior to the time considered, i.e., the apparent length of a line increases logarithmically with time (negatively accelerated growth function). When two lines are viewed simultaneously, the growth functions may differ depending on the density of number of sampled elements on each line. The second mechanism is a probabilistic mechanism by means of which sampled elements on each line are brought into relationship or are coupled *("couplage")* with elements sampled on the other line. Error is produced by unequal sampling densities and by incomplete couplings. Attenuation of error is brought about by complete couplings.

In the present review, it is only possible to indicate that this probabilistic sampling and comparison theory, though attractive and stimulating, does not further specify the sampling mechanism and makes several questionable assumptions (e.g., that only the first sampling of a line element "counts," that the probability of an element being sampled is independent of time, etc.). Piaget distinguishes four different classes of couplings: resemblance-, difference-, reciprocal difference- and resemblance between differences-couplings, but fails to indicate the principle by means of which these different classes are somehow distinguished by the subject, a principle which is needed since Piaget "explains" the law of relative centrations as the product of two coupling ratios. *Secondary Illusions and Perceptual Activity:* Piaget discusses various perceptual activities, i.e., varieties of activities of exploration, of transposition and schematization by means of which different elements of a figure are brought into relationship with each other. Perceptual activity thus results in an increase, with age, of "active" or more

"complete" couplings, and constitutes Piaget's explanation of the decrease of the systematic error in primary illusions. On the other hand, for certain illusion figures, perceptual activities may establish relationships between elements which were previously unrelated, and bring about perceptual deformations which increase with age (Oppel-Kundt illusion, etc.).

It appears that Piaget's probabilistic model leads to several difficulties all of which result from the fact that the postulated sampling mechanism of elements of a line cannot be experimentally studied without the necessary intervention of the hypothetical-correspondency or comparison mechanism. It is therefore difficult to understand effects of such variables as attention, fixation, exposure duration and distinctiveness, since both the sampling mechanism and the comparison mechanism are always involved. The same difficulty arises in the interpretation of eye-movement recordings made while the subject views illusion-figures.

This reviewer feels that Piaget's theory does not succeed in dissociating clearly the informational (sensory) input from the processing of information, an approach which not only has been used in other recent books on perception [Broadbent (1958), Dember (1960)], but which is essential for the analytical study of perceptual responses. Only future research will show whether Piaget's theory, which admits of many variables and hypothetical constructs, will have predictive value.

A Temple for Rosen?

Albert E. Scheflen

A Psychotherapy of Schizophrenia: Direct Analysis. Springfield, Ill.: Charles C Thomas, 1961. Pp. xviii + 279. $8.50.

O. Spurgeon English, Warren W. Hampe, Jr., Catherine L. Bacon, & Calvin F. Settlage

Direct Analysis and Schizophrenia: Clinical Observations and Evaluations. New York: Grune & Stratton, 1961. Pp. viii + 128, $4.25.

Reviewed by MILTON WEXLER

Albert E. Scheflen, author of the first book listed above, is a physician who is Associate Professor of Psychiatry and Director of Research in the Department of Psychiatry at Temple University Medical Center. The four authors of the second volume are also psychiatrists and also are connected with the department of psychiatry at the Temple Medical Center. O. Spurgeon English, best known to psychologists for his book with Pearson, Emotional Problems of Living, *is head, Warren Hampe, Jr. is associate, Catherine Bacon is clinical professor and Calvin Settlage is associate professor in the department. The reviewer, Milton Wexler, is a psychoanalytically trained and research oriented clinical psychologist. He practiced law for eight years before becoming involved with psychology in Teachers College, Columbia University. Interwoven with his graduate work there was experience at the University of Tennessee, in the War Department, in the Navy and in a Veterans Administration hospital. After the war and after receiving his doctorate, he served for three years as senior psychologist at the Menninger Foundation before moving to California and into private practice. He now has both therapeutic and research associations with schizophrenics; his research is carried out under an NIMH grant at the Camarillo State Hospital.*

BOTH volumes derive from observations made in the conduct of a large scale research project at the Temple University Medical Center. The subject of research is John N. Rosen's method of direct analysis for schizophrenic patients. Interest in Rosen's technique spurred the establishment of an Institute of Direct Analysis as a division of the Department of Psychiatry and a very considerable staff of assistants, observers, advisors and consultants was mobilized. Several residential homes were purchased to care for research patients, and elaborate equipment was provided for recording, photographing and even televising treatment sessions.

The basic structure of this on-going research project is straight-forward enough. Two psychiatrists and a psychologist selected schizophrenic patients for study as they were admitted to the Reception Center of the Philadelphia General Hospital. These patients had to fulfill the following requirements: (1) manifestly psychotic for less than eight weeks; (2) previously untreated by shock therapy or lobotomy; (3) between the ages of 16 and 35; and (4) in good physical health. The first patient meeting these qualifications was sent to be treated by Dr. Rosen. The next two patients were sent to two other psychiatric institutions in the Philadelphia area to operate as controls.

Dr. Rosen's therapeutic work with the patients referred to the Institute constitutes the basic data of the research. His tactics and strategies have been extensively observed, studied and discussed at regular intervals by organized panels of psychoanalysts, psychiatrists, psychologists, anthropologists and others who have aimed not only to assess the clinical outcomes of treatment but also to define direct analysis and to study its implications. The volumes above emerge as the second and third of a series of reports on this research. (The initial report came from Morris W. Brody: *Observations on "Direct Analysis"; the Therapeutic Technique of Dr. John N. Rosen.* Washington, Vantage Press, 1959. *CP,* Oct. 1960, **10,** 344.) Other volumes are projected.

Does Rosen's work deserve such intense study? In 1947 he startled the psychiatric world with a paper describing his treatment of schizophrenic patients and presenting an astonishing table indicating that his techniques had

produced remission of psychotic symptoms in 37 patients whose illness had lasted from one month to twenty-seven years. Treatment times lasted from three days to eight months. (*The Psychiatric Quarterly*, vol. 21, pp. 117-119, Jan., 1947.) Later the general technical and theoretical principles of direct analysis were presented. (John N. Rosen: *Direct Analysis*. New York, Grune & Stratton, 1953, pgs. 1-27). On the surface it would seem as if both a technique and a theory to support it existed, and that its remarkable therapeutic results deserved the most intensive study.

A study of these volumes makes that surface seem illusory. True these reports do not attempt any systematic evaluation of results and only twelve patients were in the project at the time the books were written. Even the scanty references to outcomes would suggest, however, that the clinical effectiveness of direct analysis is on a par with the therapeutic impact of a good state hospital on first admission schizophrenics. Beyond that one is impressed that all of the authors, even going back to the original report by Brody, are forced to the somewhat reluctant conclusion that Rosen's statements concerning schizophrenia are only remotely related either to what he does in fact or to any sensible conception of the schizophrenic illness.

It is difficult to understand at this point why no one of the authors, with the possible exception of Settlage, was unwilling to acknowledge that the whole project really hinged on the study of an interesting and daring personality in his relationships with schizophrenic patients. Such a project would have considerable interest and value and would have saved the authors from a certain measure of entrapment in a mainly unstated assumption that they were studying a technique, a method, a theory rather than a personality.

Once the assumption is accepted, even implicitly, that there really exists some organized theory and technique to be subsumed under the title 'direct analysis' then there exists the strong and dangerous temptation to offer a technical and theoretical rationale for the most miniscule gestures and most irrelevant tactics Rosen employs. Confusion develops out of the fusion of Rosen, the man, and Rosen as a technical and theoretical institution known as direct analysis.

On this basis every move that Rosen makes becomes too often a kind of definition of direct analysis, to be given theoretical significance. When, where, and how he uses four letter words with patients, when he is bored, when he bangs a patient's head against the floor, when he is loving, when angry, when insulting a patient, when tricking the patient, when giving the patient little time or much time, all these and more are seen characteristically as elements inherent in an established frame of reference with considerable technical value. How much easier on the authors, and perhaps on Rosen too, if there never had been such a term as direct analysis and the investigators were freer to make allowances for human strengths and weaknesses, to study a therapist as a phenomenon in his own right.

This difficulty exhibits itself throughout both books in a most general way. It can be seen explicitly in examples from the Scheflen volume. There it is pointed out that many paradoxes exist in Rosen's approach to the patient. Rosen may indicate verbally to the patient a measure of total and exclusive devotedness which he then contradicts by speaking of his investments in other patients or in his family. He may assert his maternal role with the patient but act in the most paternal fashion. He claims that "oral" interpretations are most likely to be effective but he focuses largely on oedipal or genital issues. He defines the patient's position as that of a pitiable, helpless, hungry baby in need of mothering but he often insists on adult performance. Just as often, despite his verbal presentation of himself as a selfless mother, he unhesitatingly reveals himself as a person with needs and expectations, demanding that the patient gratify him. The promise to the patient of an all-gratifying, self-sacrificing love is followed, in practice, by the most brief period of intimacy and a paternalistic, authoritarian demand for improvement.

Scheflen has certainly observed with considerable accuracy the paradoxical elements of such treatment and has implemented these observations with considerable data. However, at no point does he permit himself to see such contradictory offerings as elements of Rosen's personality or as counter-transference. He must squeeze these things into some theoretical statement and make these expressions a part of the technique of direct analysis. So he goes on to say that these paradoxical forms of behavior are merely aspects of some contrast Rosen wishes to create, first between some "crazy" pattern held by the patient and some "idealistic" pattern presented verbally by Rosen, and then implicitly between this "idealistic" performance as a mere verbal presentation and the more realistic pattern of Rosen's actual behavior.

From this it would appear as if there is some virtue in presenting such startling contrasts to schizophrenic patients, verbally offering them the moon and forcing them to settle for a cold, hard, and often painful reality in the form of Rosen's quite harsh tactics. This may or may not be so, but it pushes explanation to an unnecessary extreme. Had the investigation centered in the first place around Rosen as an interesting human being, a phenomenon in himself, it would have left some room for human error. And these books might never have been written as if the authors were in the presence of some extraordinary and mysterious achievement in which no single element could be considered as mere happenstance.

The paper by Calvin Settlage manages to escape the difficulty. He questions whether Rosen's approaches are in any way psychoanalytic and also questions whether it is possible to separate the method from the personality involved. Of all the presentations that of Dr. Settlage offers the most clarifying statements both about schizophrenia and about the difficulties, weaknesses, and dangers in Rosen's approaches. He is, however, less interested in developing a rationale for direct analysis than are the other authors, and as a critic, his job is essentially easier. His contribution is, nevertheless, most instructive and should not be missed.

Despite these difficulties one must be thankful for the effort being made at Temple University. There is no doubt about John Rosen's courage in exposing to the light of day every moment of his experience with these patients and every maneuver he employs with them. He has set more people to thinking about new ways to treat schizophrenic patients than almost anyone in this field. He has cast very considerable doubt on the emotional fragility of such patients and has given many therapists the courage to continue with problems ordinarily abandoned as hopeless.

The authors of these books have struggled hard, honestly, and often well to understand the interaction between Rosen and his patients. Their descriptions and characterizations of the process are clear, explicit and helpful. While they never succeed in presenting an acceptable metapsychology of schizophrenia which is, in turn, related to a coherent theory of technique which then in turn is correlated with specific and concrete maneuvers based on that theory, they nevertheless categorize Rosen's specific tactics very well and offer a wide variety of speculations, often very appealing, as to why the tactics may be effective. They are unsparing of Rosen, generally objective and without need to hide the differences of opinion which exist among the investigators and observers.

The Scheflen book is a more detailed, more integrated, more academic statement concerning the research on direct analysis than will be found in the book to which English, Hampe, Bacon and Settlage have contributed separate papers. The latter is basically psychoanalytic in i s orientations, and each author takes up much the same problems from somewhat different viewpoints. Both books attempt to give some definition to the treatment tactics characteristically employed. Both make some kinds of judgments as to the consistency, value, appropriateness and theoretical meanings of these tactics. The considerable overlap among the authors of both books seems quite justified since precisely in the differences of opinion which arise one can often see a sharpening of issues and understanding. This might not be true if a greater separation of areas for consideration had been attempted.

Despite all the difficulties inherent in this research it seems without question that the hope expressed by O. Spurgeon English, will be easily realized; that these offerings may prove sti.nulating to others.

Und-verbindungen on Coercive Persuasion

T. H. Pear

The Moulding of Modern Man: A Psychologist's View of Information, Persuasion and Mental Coercion Today. London: George Allen & Unwin, 1961. Pp. 220. 21s.

Reviewed by Edwin G. Boring

T. H. Pear (rhymes with beer, not hair) is now Professor Emeritus at Manchester University. He came to Manchester in 1909, after study at the University of London, at Würzburg and at Geissen, and served ten years as a lecturer before becoming Professor of Psychology. Between the years 1909 and 1960 his bibliography grew from zero to 94 items. The present book makes at least 95. His writing has been concerned with man—his skills, memory, voice and conduct—in a social setting. Edwin [de la] Garrigues Boring, the reviewer, is known to a number of American psychologists. He was born, as was Mach's Analyse der Emfindungen, *in 1886, and entered psychology after his thoughts were reformed by Titchener at Cornell University. He moved to Harvard in 1922 and has been there, with constantly increasing scope, level and significance of activity, ever since. He became Emeritus in 1957 and on January 1, 1962, became a very lively and the only living ex-editor of* CP. *Those who may have heard of him and want to know more can read all about it in his autobiographical book,* Psychologist at Large, *published by Basic Books in 1961.*

More and more, with the culture's coming to regard man as controlled and therefore controllable, does the invasion of man's privacy of action and motivation increase. Just that is the manifest theme of this little fact-packed book, and the latent theme is that the author, Pear, does not like what is going on; he would like to see the trend resisted. He does not say so right out but he must average an innuendo per page that gives him away exactly as he wants to be given away. Though the book be fact-packed, it is steeped in values. It is not, however, a roaring conservative protest against encroachments on man's freedom, but rather a steady unilateral conversation by a bright dinner companion, one who is chock full of information, who spices his facts with quips and humor, and who stirs up no more commotion than comes from the constant raising of his eyebrows.

Pear starts out with an encomium for the self. Here he plays James regretting Wundt, or Henry Murray regretting the sensation-perception psychologists that he found so firmly entrenched at Harvard when he arrived in 1927. What do the "prism and pendulum psychologists" (that's James) know about "beauty, charm, gaiety, wit, humor, and bonhomie" (that's Pear)? And how big is your self? Does it include your clothes? Was the bobby's privacy invaded when he was made to shave his beard off? May your self include your car with which, when driving, you are one? or your plane? In any case the self is a social self, disintegrating when held in complete isolation.

The main body of this almost wistful defense of freedom and privacy is the six

chapters on intellectuals, public opinion, loyalties, treachery, brainwashing, thought reform, advertising, publicity, and mass culture. The best chapter is the thirty-two pages on "Brainwashing" and "Thought Reform," and the second best the thirty-five pages on "Advertising and Publicity." Together they make up a third of the book. Each is a connected discussion, and the account of what recent reports show to be the Chinese methods of thought reform is illuminating and thought-reforming for the average western intellectual.

The book is well organized into chapters, and no paragraph ever contains unrelated facts, yet I find the reading fatiguing, because the talk does not flow smoothly. Perhaps there are 800 facts in this book, four per page, but a dozen on certain pages. Could the author have been collecting these on cards for five years, have now sorted them out by chapters and put related ones near each other, and then have written his text, letting his facile mind play with the sentences as it sneaks the values in? Are there, maybe, a dozen other orders that would have been just as good? I want topic sentences for these paragraphs, not just two or three facts in juxtaposition with the transitions left to the reader's wit. I want five or six hundred more connectives. I want *Struktur* in the chapters, not mere *Undverbindungen*. I want the chapters to march, not merely to wear out the grass by marking time.

I found only one typographical error, which I mention for fun because it is the author misprinting his own name ("P. H. Pear" for T. H., p. 169). Or did someone else slip this sentence in? It is hard to imagine Pear wearing the envelope of his self so tightly wrapped that his initials stick through to get injured in transit.

The book provided me with one big disappointment. For years I had been wondering why there is all this excitement about hidden persuaders and subceptive advertising when the great instigators of subception are parents, schoolteachers and college instructors, as well as the *Zeitgeist*. What are we worrying about? I thought Pear would tell me and he did not. He is for freedom and privacy, but these are negative

T. H. Pear

words. He is not for the supreme privacy of complete social deprivation. In general, the invasion of privacy is a positive thing. Has Pear only negatives with which to oppose it? He wants a self that is not too big nor too compressed. Is he possibly just against change? Hardly, for he is voluble on the point that not all change is progress; yet is it not true that all animal organisms need activity and thus change? One begins (say I) to understand life when he perceives that man strives always for adaptive adjustment and then welcomes disadjustment that he may strive again, since health is activity.

But not everyone will be frustrated by Pear's failure to take on the battle between freedom and determination. There are plenty of readers who want only to be told how it is that man can be defrauded of his freedom when he does not welcome foreign control. Pear's book is full of interesting facts and leaves the reader with the privilege of deciding what might be done about them. Read the book and then be human: decide how private you think the world really is, how much of its seeming privacy has been the delusion of susceptible (subceptive) mankind, and then what you would really like to see changed about the past and this not-so-different present. (And do not forget your principal, always active employer of subception: the *Zeitgeist*.)

Equalitarian Dogma?

United Nations Educational, Scientific and Cultural Organization

Race and Science. New York: Columbia University Press, 1961. Pp. 506. $5.00.

Reviewed by Thomas F. Pettigrew

The contributors to this volume include the well known American psychologists Otto Klineberg and Marie Jahoda. Other prominent American contributors are the sociologist Arnold Rose, the zoologist L. C. Dunn and the anthropologist Harry L. Shapiro. From other countries there is Juan Comas of Mexico, Kenneth Little of Scotland, Michael Leiris and Claude Levi-Strauss of France and G. M. Morant of England. The reviewer, Thomas Pettigrew, is a Virginian with a Harvard PhD who is now assistant professor in Harvard's Department of Social Relations. He was co-author of the 1959 book Christians in Racial Crisis: A Study of the Little Rock Ministry *and is now conducting studies of Negro personality and is writing a sociopsychological analysis of desegregation in the South.*

What does modern science say about the concept of race? What does it say about the nature of prejudice? And what does it say about the persistent theories of racial superiority?

These questions are of such obvious importance throughout the world today that UNESCO began to publish in 1951 a series of brief, popularly-written essays that offered documented answers by leading authorities. The series, entitled *The Race Question in Modern Science*, ran to eleven monographs and tackled the questions from a wide variety of approaches: anthropological, biological, historical, psychological, and sociological.

Now the entire series has been republished in this single volume. No new material is introduced, nor is any continuity or index provided; but the essays, each complete in itself, range over a vast array of situations and data,

supplementing one another in an impressive fashion.

Three papers are of special interest to psychologists. Sociologist Arnold Rose writes on "The Roots of Prejudice," though he has argued elsewhere that prejudice as a concept is of limited value in understanding race relations. While his essay presents interesting examples of the operation of prejudice in history, its discussion of personality factors is thin and outdated—indeed, inaccurate in places. Authoritarian personality research, for instance, is relegated to part of one paragraph and identified "as a type of mental disease" (p. 417).

More pertinent to the problem of prejudice and personality is Marie Jahoda's psychoanalytically-oriented essay, "Race Relations and Mental Health." She refutes the contention that authoritarianism is "a type of mental disease" by an instructive review of the relevant research. Instead, Jahoda makes a sharp distinction between the absence of mental illness and positive mental health; she persuasively interprets the available evidence as indicating that deeply prejudiced individuals are no more likely to be mentally ill than tolerant individuals but are less likely to possess positive mental health. The forceful, tightly-reasoned style of Jahoda's paper makes it a classic presentation of the Freudian interpretation of prejudice, a presentation ideal for assignment to undergraduates.

The third psychological paper has historical significance for the discipline. Up through the 1920's, the vast majority of race studies in psychology supported the theories of racial superiority. Empirically inadequate and theoretically naive, the race literature of the period was characterized by intelligence test studies of white and Negro Americans that interpreted the usually higher white IQ's as evidence of inherent intellectual differences between the races. The 1930's witnessed a sharp change. Though still crude in some respects, a new series of more rigorous race studies appeared in the psychological literature. Emphasizing environmental factors, they attempted to answer racists' claims. In this sense, there was a defensive quality to these newer studies; but less defensive, more carefully controlled research of the past two decades has replicated and confirmed the basic findings and conclusions of the 1930's.

THE MOST prominent contributor to this literature, Otto Klineberg, sums up this work in his paper, "Race and Psychology." He makes a clear and straightforward case for the dominant environmentalistic position of modern psychology; concentrating on racial studies in intelligence, he unfolds the reasoning behind why racist claims receive such little attention among current psychologists. He does not deny that inherent psychological differences *may* exist between races, but he argues that racial differences noted so far can be easily explained in environmental terms. Rigorous empirical demonstrations of racist claims simply do not exist; while findings predicted by environmentalists continue to be obtained.

Though Klineberg's points are quite familiar and widely accepted, the need of their restatement is demonstrated by the resurgence of racist thinking since the 1954 Supreme Court desegregation rulings. Even within psychology, a few writers have returned to 1920-like arguments of Negro inferiority, and their views have received wide attention in the mass media. They hold that the majority of psychologists have prematurely closed the issue for ideological reasons but do not discuss their own motivations. Indeed, Klineberg's UNESCO paper is a frequently cited example of what is perceived to be the politically-inspired leanings of the profession. One psychologist, Professor Henry Garrett, has gone so far as to assert that the "equalitarian dogma" concerning Negro and white intelligence is "the scientific hoax of the century" (AP dispatch, July 29, 1961).

This is a grave charge. Psychologists and their students might well read Klineberg's essay and Garrett's answer (in *The Mankind Quarterly* of July, 1960), and decide for themselves if anyone is perpetrating a hoax.

1984 Is Not Yet

A. D. Biderman and H. Zimmer

The Manipulation of Human Behavior. New York: Wiley and Sons, Inc., 1961. Pp. xii + 323. $7.50.

Reviewed by T. W. MILBURN

The editors and author of the present volume are identified in the review. T. W. Milburn, the reviewer, is a Stanford PhD who has worked as evaluation and training supervisor for the Dalmo Victor Company, has served as counselor and researcher in the Stanford Counseling Center and, since 1957, has headed the Behavioral Science Group at the U.S. Naval Ordnance Test Station at China Lake, California, where he pursues his interest in group processes, cognition and international relations.

AMERICA'S concern with "brainwashing" began with the Korean War and has provided impetus for various more or less scientific investigations and several books on the subject. *The Manipulation of Human Behavior* is certainly one of the least polemical of these. Written with scholarly care and edited by behavioral scientists well-acquainted with the fields involved, it is a book for the scientific community.

Editor Biderman, a sociologist trained at the University of Chicago, has had a number of articles published recently on behavior in captivity situations. Editor Zimmer was trained in clinical psychology at the University of Rochester, and his current work is directed toward the automatic translation of physiological response measures for a digital computer. Both collaborated earlier on a study of Communist exploitation attempts against Air Force personnel captured in Korea.

The authors are mostly psychologists, although two—Louis Gottschalk, who writes on the employment of drugs, and Lawrence Hinkle, who covers the effects of physiological state on brain functioning — are psychiatrists. One,

Martin Orne, ("The Potential Uses of Hypnosis in Interrogation") has both an MD and a PhD. R. C. Davis of Indiana University has experimented with and written a good deal on the subject of lie detectors. Philip Kubzansky is also a co-editor of the ONR symposium, *Sensory Deprivation*. Robert Blake and Jane Mouton have experimented extensively on social influence and conformity behavior, often most ingeniously.

Oriented to the search for dependent variables, the book consists of a group of reviews which deal with objective and mostly non-clinical studies of various antecedents of what is in effect cooperative behavior with interrogators. It is concerned neither with incidents of brainwashing *per se* nor with descriptions of the characteristics of resistors or cooperators. Instead of dealing with differences in individual ability to resist, it is directed more to the explication of situations or situational variables that might induce *effective* (from the point of view of the interrogator) cooperation, whether coerced or not.

The antecedent causal chains are explored through separate chapters on the use of drugs (137 bibliographical items), sensory deprivation (81 items), hypnosis (86 items), physiological stresses (136 items), and interpersonal influence (134 items).

There is an excellent chapter on "physiological responses as a means of evaluating information," reviewing the effectiveness of measures that interviewees might employ against interrogators and the extent to which interrogators can easily evaluate the fact that these are being utilized (92 items), as well as an excellent, conservative introductory chapter by the editors (69 bibliographical items).

BIDERMAN and Zimmer attempt to summarize the book in their introductory chapter, pointing out that the "conclusions reached [by the various reviewers] do in fact show that many developments can compound tremendously the already almost insuperable difficulties confronting the individual who seeks to resist an interrogator unrestrained by moral or legal scruples. On the other hand, it can be shown that *many of the measures popularly supposed to render an interrogator omnipotent actually have no demonstrable applicability to his purposes*" (page 9).

They also touch nicely upon anxieties in the area of behavior control:

"Two of the most basic of life's problems are linked to the individual's power position *vis-a-vis* his fellow men: the inability to make others fulfill one's wishes; and the reverse, the fear of being controlled by others with the consequent loss of the autonomy that is believed to be fundamental to the conception of the self" (page 4).

"The purposes that men have in seeking to control or to influence the behavior of others involve the distinctively human capabilities of men and their significance for one another" (page 7).

Strictly speaking, Freud's great contribution was his creation of a procedural framework for inquiring into a two-person situation, especially into transference and resistance. If psychoanalysis is not to become an outmoded system of interpreting human experience, with only a dusty niche in the history of ideas, then the vitality and scope of its method for studying this subject matter must be continually strengthened. It stands or falls as a psychological method of gaining insight and knowledge about human relatedness and communication.

—*Benjamin Wolstein*

The editors considered for inclusion and then omitted a chapter on the relevance of subcortical stimulation to problems of interrogation, citing a report by Colonel Sidney Marvin, MD, of Walter Reed: "Unless knowledge and techniques exist far more advanced than those known to the Western scientific community, intracranial stimulation known would not be employed in any rational attempt to elicit intelligence." Also omitted is any consideration of the role of Pavlovian conditioned reflex theory on the basis that it is too simple to explain the phenomena in question, a conclusion with which Bauer (1957), for example, would certainly agree.

Space prevents a review of each chapter, but each is full of interesting information. Davis, for example, suggests that a variety of psychological techniques is likely to be more effective for detecting lies than a single one because a person being interviewed habituates to each at different rates, and each person has a kind of signature among these (his own idiosyncratic response pattern). One person under pressure will show much change in galvanic skin response (GSR), for example, and very little change in blood pressure while another will show changes in blood pressure and very little change in GSR.

The reviews as presented suggest that those stressful measures that can elicit cooperation usually do so at the expense of a reduction of the *capacity* to cooperate, especially at complex tasks. It appears, moreover, that some persons can tell undetected lies despite drugs or lie detectors. The data suggest, too, that stress is not likely to be a successful antecedent of ideological conversion, that the two concepts are cognitively dissonant with one another. One is reminded that subliminal preception, which not long ago aroused anxieties about control against one's will, appeared overly impressive for a little while.

This reviewer wishes the editors had asked each author to use a similar set of criteria for evaluating the state of knowledge in this area or that they had attempted a final integrative, synthesizing chapter, employing some concepts from areas such as decision theory. At the same time, however, they must take credit for the calm, dispassionate and objective tone of the whole book, as well as for its clarity.

The book should prove a useful supplementary test for advanced, broadly-oriented courses in social psychology or personality. In addition, the various chapters could do excellent duty as points of entry to the subject matters they treat.

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Prologue to a Theory of Thought

Joseph Church

Language and the Discovery of Reality: A Developmental Psychology of Cognition. New York: Random House, 1961. Pp. xviii + 245. $4.00.

Reviewed by WILLIAM KESSEN

Joseph Church received his MA from Cornell and his PhD from Clark University and since 1954 has been on the faculty of Vassar College. In 1957, with Joseph Stone, he co-authored Childhood and Adolescence (CP, *May 1958,* **3,** *132-139). William Kessen came from Florida to Brown to Yale and on into psychology. He received his PhD at Yale in 1952 and has been there ever since, first as a USPHS Fellow and then as a member of the faculty in the psychology department and in the Child Study Center. His primary research interest has been centered on emotion, learning and cognitive development in the human newborn.*

THE psychologist who writes of language and of cognition in 1961 shows timely courage. The moment seems right, because several related streams—linguistics, the computer simulation of human thought, Piagetian studies, a renewed interest of psychologists in research and speculation about language—press toward confluence. But the timeliness of an inquiry into the origins of cognition is fairly matched by its difficulty; diversity of facts, incongruity of theories, and great chunks of raw ignorance face the man who would comprehend the acquisition of human knowledge and speech.

Professor Church has the requisite courage; in the startling words of his preface, *Language and the Discovery of Reality* "is about the way human beings, in the course of growing up, come to discover reality: people, things, attributes of people and things, space, time, values, opinions, ideas; their own bodies, feelings, wants, needs, and intentions; and the multivarious functional and formal relations among all these." Judged against this statement of intention or against the promise of title and subtitle *(A Developmental Psychology of Cognition),* the book falls short; but in falling short it illustrates how far from anecdote and aphorism the psychology of human thought and language has come in recent years.

Language and the Discovery of Reality is written with clarity and occasional grace but it is diffusely organized. Themes that appear regularly are a critical account of the behavioristic views of language and thought, a presentation of Professor Church's theory of preverbal experience and language acquisition, and a discussion of research methods in the study of cognition. Each of these themes is developed in a way that provokes the reader to interest—if only the interest of contest—and then, just as he anticipates the expression of a new uniting insight or the inescapable impalement of an S-R-reinforcement theorist, the argument often drifts off toward inconclusive commentary.

The shape of Professor Church's procedure can be illustrated by the case of the behaviorist and language learning. It may be true that, under the attack of Chomsky and in consideration of the work of Roger Brown and his colleagues, any simple application of principles derived from the learning of topographical responses to the learning of language can be called into question.

JOSEPH CHURCH AND FRIENDS

But Professor Church does not depend on a systematic analysis of data or of theory to reject a reinforcement analysis of language learning. Rather, he convinces us of the complexity of the young child's speech by particular examples; he puts words into Skinner's mouth

> ". . . When Skinner speaks of 'relatively unpatterned vocalizations' he apparently assumes that the baby emits random clusters of miscellaneous speech sounds,"

knocks them out with vigor

> "It is quite obvious, to anyone who takes the trouble to look, that the child does not piece together words out of phonetic elements, but grasps and utters them globally . . ."

and then smites the enemy vitally

> "There is abundant experimental evidence showing that learning occurs in the absence of specific motivation and reinforcement."

With the field thus cleared Professor Church makes the following substitute proposals.

> ". . . the child learns to respond to requests and cue words without specific training and without reward; at a given point of experience, he simply knows what they mean."

> "For the child's first unequivocal speech comes not as a product of parental prompting but when, without warning and for no discernible reason, the child one day points to something and says its name."

> "But in spite of the substantial amount of learning by imitation, the greater part of the child's vocabulary is learned by simple absorption."

Disenchanted as psychologists may be with available theories of language acquisition, it seems unlikely that adequate substitutes will be found by such a rejection of psychological analysis.

HARDLY less disconcerting are Professor Church's proposals for a theory to encompass cognitive development. If Skinner is the antagonist of this book, Werner is its hero, and there is acknowledged evidence of his contribution to *Language and the Discovery of Reality*. In addition to the familiar notions of physiognomic perception, egocentrism, empathy, and participation, Professor Church builds his version of human thought around the constructs *organismic mobilization*, *schematization*, and *thematization*. The action and perception of the very young child require "total organismic mobilization"—at this stage, "to perceive is to unify oneself with the object." But as perception becomes autonomous, and the preverbal child learns about the world, he builds schemata—the "implicit principle(s) by which we organize experience." With the acquisition of language, the child can thematize his knowledge; he can reduce its complexities "to manageable symbolic forms." The result is an ability to deal with reality abstractly and fluidly—"full consciousness of reality is possible only to the extent that it has been verbalized"—but the categories of language also exercise control over the child's schemata, as perceptual experience "is subordinated to a whole conception of reality, a conception built up out of countless concrete events, verbalizations, and verbalizations about verbalizations, whereby successive environments are woven into a world."

This barbered account of Professor Church's position does not represent the easy flow of his narrative or his use of engaging detail about children but, putting expository advances aside, this account of cognitive development hardly goes beyond Werner or indeed as far as Piaget. It is a plausible statement, never far from the language of common sense, about the life of the child; not surprisingly, this sensible presentation entails a reluctance of the author to go beyond sense to systematic order. Professor Church has an unclouded appreciation of the subtleties and rich variation of children's thought and he sets tasks for the cognitive theorist with his descriptions of developmental phenomena, but both his commitment to the particular and the vastness of his vision jeopardize a systematic analysis of the facts. In addition, a chapter on methods of research and paragraphs on a number of themes scattered through the book are not brought into close fit with the central issues and these passages often interrupt the pattern of presentation.

Language and the Discovery of Reality—partly because Professor Church ranges so widely and attempts so much, partly because anti-associationism is not an adequate replacement for an imperfect associationism, partly because a promising period of research in cognitive behavior is just beginning—may most properly be considered a prologue to a developmental theory of cognition.

Psychopathology all Over

Joseph Zubin (Ed.)

Field Studies in the Mental Disorders. (Proceedings of the Work Conference on Problems in Field Studies in the Mental Disorders, Feb. 15-19, 1959, under the auspices of the American Psychopathological Association, USPHS Grant No. 3M-9146, NIMH.) New York: Grune & Stratton, 1961. Pp. x + 495. $6.75.

Reviewed by HENRY P. DAVID

Joseph Zubin, the editor of this volume, is a prolific author and is well known to many psychologists of many kinds. He is now professor of psychology at Columbia University and, at the same time, is Principal Research Scientist in Biometrics of the research minded New York State Department of Mental Hygiene. Henry P. David, a clinical psychologist with a PhD from Columbia, has served since 1956 as Chief Psychologist for the New Jersey Department of Institutions and Agencies. He combines an interest in clinical psychology with an interest in international affairs, has helped edit two volumes on personality published under the auspices of the International Union of Scientific Psychology, has traveled recently in Russia and Africa and has done five earlier reviews for CP.

WITH the support of the National Institute of Mental Health, the American Psychopathological Association organized in February 1959 a four-day Working Conference on "Problems in Field Studies in the Mental Disorders." The Program Committee, chaired by Joseph Zubin, enticed 38 eminent individuals from the United States and Europe, representing a broad range of disciplines and investigative approaches, to engage in a rich schedule of morning, afternoon and evening sessions, fully transcribed in the Proceedings here published.

A reader selecting his purchases from book titles only, without awareness of the lengthy subtitle, may well be surprised. The focus of the Conference was on problems, not field studies. After the keynote address by philoso-

pher Carl G. Hempel, discussion centered on taxonomic problems of nosology and nomenclature. For the next two days participants considered problems of definition, units of study and variables, and comparability in field investigations of general health and/or mental disorders. The last day featured a marathon of nine summaries, each from the viewpoint of a particular discipline, followed by summary comments and a final Summary of the Sessions. Indeed, about 25 percent of the Proceedings is devoted to varied summaries, digests of digests and reminders to participants of different approaches to general goals. It was an organized Conference.

Ably edited by Joseph Zubin, the Proceedings reflect the stimulating Conference atmosphere, complete with indications of laughter and applause. While there is some inevitable meandering and retracing of steps, the sense of participation is lively, projecting the problems of differing terminologies, nosologies, theoretical biases, and classification concepts. And, if that is not enough, the reader is invited to dig further in a 68 page appendix of 84 tables and 225 references, presenting international core data pertaining to class, ethnic, rural-urban and geographic mobility correlates of illness, compiled by Muriel Hammer and Eleanor Leacock.

The lack of an accepted and widely applied classification of mental disorders is a serious obstacle to epidemiological research and international communication among the mental health professions. In his summary, sociologist John Clausen noted that several subcultures were represented at the Conference, reflecting different sets of beliefs, values, and goals. Practicing psychiatrists in understaffed facilities can hardly be expected to regard diagnosis and classification with the detached research approach of biostatisticians and epidemiologists. And, as the Proceedings show, there are considerable differences in definitional and conceptual approaches among psychiatrists. For example, studies reported by Scandinavian participants were carefully designed to elicit genetic linkages while using administrative data as a basis for socio-cultural classifications. In contrast, American studies generally attempted to test hypotheses of social influence, frequently neglecting genetic dimensions. More integrative interdisciplinary and international contacts are clearly desirable if the suspected interaction between genetic and social characteristics and influences is to be fully explored in future studies.

Conference discussions quickly confirmed that neither the *World Health Organization International Statistical Classification Section on Psychiatry* nor the American Psychiatric Association's *Diagnostic and Statistical Manual* have attained wide or reliable acceptance. With the lack of agreement on psychiatric nomenclature and diagnostic concepts, it is hardly surprising that so many research studies flounder on the unreliability of diagnoses, even within the same institution. Perhaps, as biostatistician Morton Kramer put it, we have to decide what questions we want answered and proceed from there. And, as WHO Consultant Stengel noted, an international statistical classification must be "a servant of international communication rather than its master."

With the rapid emergence of new countries and the shortage of trained professional manpower, the most desirable international classification will likely be one that is readily applicable anywhere. As WHO Mental Health Section Chief, Dr. Eduardo Krapf, observed, "no classification will be used unless it's within the practical possibilities of the average psychiatrist to handle it easily." It follows that any proposed system should begin with operationally defined diagnoses, unburdened by theoretical and nosological biases. Only after reasonable reliability of diagnoses has been achieved, will the reports of incidence and prevalence have any real meaning for purposes of comparison from state to state or country to country. Adopting a simple international classification does not mean the abandonment of other systems, such as that developed by the American Psychiatric Association. The only proviso should be ready convertibility, which in some instances has already been demonstrated.

There was general recognition at the Conference that diagnoses are more than private observations by individual psychiatrists. While it is unlikely that any international classification can be developed to meet every criticism, not even the best can serve its purpose unless those participating in its application want it to succeed and work effectively toward that goal. Reliable communication is essential if social scientists wish to counteract ethnocentric tendencies in the establishment of behavioral norms and develop a sound basis for cultural comparisons.

What is the significance of this interdisciplinary conference for psychology, and more particularly, for clinical psychology? There was a general concensus that further international discussions of problems are likely to be of little avail. The time has come to initiate actual field studies, directed by skilled experts, including psychologists, representing different cultures and varied approaches, determining areas of agreement and disagreement, and arriving at a general concensus for a concerted international approach. There would be no better beginning than implementation of Zubin's suggestion to use registrants for military conscription in the NATO countries as comparable populations for epidemiological field studies. It may well be asked whether clinical psychological training programs are currently producing enough graduates prepared and interested to contribute to an understanding of broad public health import?

Mental disorders respect neither geographic, linguistic, nor disciplinary boundaries. The Proceedings reflect common problems and aspirations. As such, the volume offers a unique introduction to the complexities of epidemiology, a rich resource for graduate seminars in mental health, and must reading for clinical faculties reviewing training goals.

A man is a bundle of relations, a knot of roots, whose flower and fruitage is the world.

—Ralph Waldo Emerson

Old Order Changeth?

Jules H. Masserman (Ed.)

Principles of Dynamic Psychiatry. (2nd ed.) Philadelphia: W. B. Saunders, 1961. Pp. xxii + 332. $8.00.

Reviewed by IVAN N. MENSH

Jules Masserman, psychiatrist, researcher, prolific author and busy editor is Professor of Psychiatry and Neurology at Northwestern School of Medicine. The reviewer, Ivan Mensh, besides serving frequently as a reviewer for CP *does a variety of other productive things. He is Head of Medical Psychology in the School of Medicine at the University of California at Los Angeles, to which position he moved in 1958 from a similar one at Washington University. He has had a long history of responsibile involvement as a consultant in governmental research advisory groups and, as a participant, in APA boards and committees (E & T Board, the old CSPA, BPA, etc.). While a graduate student at Northwestern he sat in on several of Dr. Masserman's seminars.*

ONE of the criteria of the courage of an author may be found in his releasing a second edition. The scientific reading audience is likely to make comparisons with the earlier works of the author, perhaps even re-read the earlier edition. However, a span of 15 years as in Masserman's *Principles* may be an interval long enough to allow for a significant change in the reading public, so that old-timers are relatively few, and those with adequate recall even fewer. A comparison of selected phrases affords an interesting insight into Masserman's perceptions of the changing trends over the past decade and a half. (See box below.)

Masserman's own perceptions of the changes in psychiatric principles and knowledge emphasize neurophysiology and pharmacology, comparative and experimental psychology, social psychiatry, and psychoanalysis. Also, in place of discussions on combat neuroses and wartime morale, he has introduced sections on ethology, information and systems theory, disturbances in communication, and the evolution of psychotherapy.

As in the earlier edition, the question still is unanswered why either edition represents" . . . an Integrative Approach to . . . Psychology." The principles, observations and comments appear primarily designed for students in the health disciplines, e.g., medicine, nursing, social work, and psychology; and it has been a mystery to the reviewer how these approaches are to psychology and not to psychiatry. The approach will not be accepted by many psychologists as integrative when they inventory the illustrative experiments—18 from Masserman's laboratory, all with cats; Cole's racoons; Finch and Culler's dogs in their signal substitution experiments; and a sole experiment with human subjects, Hudgins' verbal "commands" in conditioning pupillary contraction responses. The author does specify his definition of the integrative approach by indicating the motivation for his texts. He states his "interest in a possible resynthesis of behavior theory with the biologic sciences, in the supplementation of medical and psychiatric observations by direct animal experimentation." The latter are best exemplified and known through the studies of alcoholism and neurosis in cats at Masserman's laboratory.

The organization of the contents into historical, comparative and experimental approaches to the study of behavior represents a useful introduction. Further, the author's style and the larger print of the second edition make for easy reading, but there also appears the danger that the naive student will accept uncritically the dogma presented, for example, the too-easy generalization to human behavior from the cat experiments in "alcoholism" and "neurosis." Further, the student may be frustrated by reading of "Gildea's control studies on 'taraxein'," only to find that there is no reference to this at chapter's end or in the bibliography. This is understandable only if one knows that the studies were done by Robins et al, in Gildea's department, it is true, but cross-referencing usually does not include such allusions.

Finally, Masserman's development of his "biodynamic theory of behavior" not only summarizes his organization of psychiatric principles, but also introduces a series of criteria which he hopes are met by the theory he proposes. These represent his attempts to integrate data on evolution, anatomy, physiology, environmental and organismic influences, behavioral variations in both genotypes and phenotypes, and pragmatism. The criteria are excellent ones and have relevance to other theorists than Masserman alone, but their comprehensive nature also foretells that no single theory can at this time encompass so much.

1946	1961
Principles of Dynamic Psychiatry: Including an Integrative Approach to Abnormal and Clinical Psychology.	. . . *Approach to Experimental and Clinical Psychology.*
"Psychiatry, the Science of Behavior."	"Psychiatry, a Science of Behavior."
"Biodynamic Correlates of Current Theories of Behavior . . ."	"Existentialism and Phenomenology, and Communication Theory."
"Principles of Conflict."	"The Effects of Conflict."
Appendices: "Sane principles of group communication."	"Ethology and biodynamics."
"Current schools of psychology (e.g., Lewin, Tolman, Hull) . . . ."	". . . Lewin, Tolman, Hull, J. G. Miller . . . ."

A Therapist's Theory of Thinking

Harley C. Shands

Thinking and Psychotherapy. Cambridge, Massachusetts: Harvard University Press, 1960. Pp. xvi + 319. $5.75.

Reviewed by THOMAS C. GREENING

Harley C. Shands is professor of psychiatry, and Director of the Psychiatric Research Center at the Downstate Medical Center (in Brooklyn) of the State University of New York. He came into psychiatry by way of surgery, internal medicine and then psychoanalytic training. The reviewer, Thomas C. Greening is a clinical psychologist who had his graduate work with Fred Wyatt at the University of Michigan and who now is happily busy, mostly with psychotherapy, as a partner in Los Angeles' Psychological Services Associates. He is psychoanalytically oriented but admits to some deviationistic tendencies in an existential direction.

THIS BOOK attempts to integrate current theories in neurophysiology, linguistics, social psychology, and psychiatry. It is an exciting effort to establish continuity and synthesis among these different approaches to the study of man.

With an urbane guide who delights us with illustrative references to Cassandra, Fangio, and Ustinov, we journey from Sherrington to Riesman, by way of Freud, Dewey, Piaget, and Whorf. The author states his purpose clearly and proceeds confidently and systematically. The table of contents and the introductory chapter provide an explicit and reliable guide to what the reader may expect.

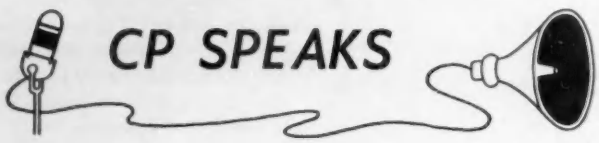
While the general style and organization makes this a very readable book, the preponderance of flat, declarative sentences is a discomfiting stylistic element in an avowedly theoretical and tentative presentation. There is a scholarly confidence and directness in these assertions, but they lack the quality of engaging invitations to the reader to join the author as he progresses from observation through hypotheses and discussion to conclusions. The author seems to prefer to lecture the reader rather than entreat him to collaborate in developing a thought. He is a most competent lecturer, but some readers may feel rather coerced.

In language, concepts, and exposition, the content will be comprehensible to any PhD psychologist. It will be of at least some interest to all psychologists who enjoy seeing personality theory extended and deepened. The advanced theorist will find nothing new or unusual, but may often be pleased by a nicely presented integration of data and theories or a new slant on the mind-brain problem. Although half the title refers to psychotherapy, the author does not attempt to present a substantial discussion of psychotherapy. The psychotherapeutic interaction is used repeatedly and effectively, however, to illustrate the discussion of thinking. This discussion does not present sufficient richness of clinical data or theory to appeal to an already well-grounded psychotherapist looking for new insight into his work with patients. He will, however, gain a greater appreciation for ways in which the specific practice of psychotherapy can be conceptualized with added meaning when placed in the context of modern theories of thinking.

The excellent discussion of language, perception, and complex thought processes has great relevance to modern psychoanalytic ego psychology. Readers interested in this area will find many sections of the book quite pertinent, particularly the extension of Piaget's concepts of accommodation and assimilation. The implications for ego psychology are not brought out by the author, however, in spite of his professed interest in psychoanalysis. One passing reference is made to Hartmann's work, but other major contributors to clinically-derived theories of thinking, such as Kris or Rapaport, are not discussed. Similarly, much in this book has central significance for concepts of identity and self. The author recognizes this: "The background upon which human behavior always rests is the mechanisms of identification by means of which one's identity is . . . established," ". . . the central problem in reflective thinking is the self." Here also, however, many readers may be disappointed in the lack of connecting links with relevant theories. Fairbairn for example and Klein are mentioned, but only in passing, and no consideration is given, for example, to the ideas of Erikson and Spitz.

Perhaps it is asking too much of an author that in the process of such an ambitious synthesis he manage to include all relevant major theorists. Because he does so much so well, we expect him to do everything. Nevertheless, these omissions and the sketchy treatment of psychotherapy produce a somewhat anti-climatic effect. The title, *Thinking and Psychotherapy,* is so impressive, the introductory chapter so systematic, and the style so confident, that one is led to expect a more monumental, constantly evolving synthesis. Instead, there seems to be a lack of follow-through or summarization at the end of chapters. This is especially disconcerting in the final chapter, when, after an interesting but brief review of defense mechanisms from the author's point of view, the book suddenly stops.

The abstract discussion of psychotherapy might have been enlivened with excerpts from transcripts. Piaget, to whom the author acknowledges his debt, has shown the feasibility and value of combining verbatim process recording with theoretical discussion, as has Rogers. Shands does use as examples several cases and one sequence of images reported by a patient. In general, however, the book remains at an abstract level. Readers who find the theory interesting but who want more documentation will be pleased to know that the author is presently engaged in a research project attempting to study operationally, by means of taped interviews, patients' shifts in levels of abstraction during the course of psychotherapy.



Gestation of CP

Not many of *CP's* readers know about the conception, gestation, and birth of the Journal. Neither did the present editor know about such things, except hazily, until some recent thumbing through back issues of the *American Psychologist*. A prenatal history is there; but as in the case of a human organism, many significant events and processes before birth are not fully recorded.

Who first had the seminal notion of a book review journal? There is no published record. It is possible that the APA Publications Board of 1952 discussed the matter. It is clear, though only inferentially so, that the 1953 Publications Board did discuss the matter despite the fact that the *American Psychologist* of that year carries no mention of it in any report of the proceedings of the Association. The following were members of the 1953 Publications Board that met in Washington in the spring and again in September in Cleveland: Herbert S. Conrad, J. McV. Hunt, Wayne Dennis, Paul Farnsworth, Joseph Shoben, Jr., William H. Hunt, I. E. Farber, Harold Seashore, and Robert L. Thorndike; Carroll Shartle, APA Treasurer, and Fillmore Sanford, Executive Secretary, were present as *ex-officio* members. The conception of the journal has to be traced to this group or, if one is repelled by the idea of group fatherhood, to some member of it. The last named member of the group reports with certainty that no one could, with success, either credit or charge him with paternity. One member of the 1953 board has the distinct memory that Edward Girden was asked by the board to head a special task force to study the project.

That this group did discuss the idea of a book review journal and did find it a nutritious matter is indicated by the following screed written by the Association's Executive Secretary and published in January, 1954.

> For the past year the Publications Board has been discussing the proposition that the APA start a new journal devoted primarily to reviews of psychological books, films, tests and perhaps apparatus.
>
> At the moment, four of our journals carry book reviews. The space devoted to book, test, and film reviews in 1952 was as follows:

	No. of Pages	% of all Pages
Psychol. Bull.	121	26.5
J. appl. Psychol.	38	8.8
J. abnorm. soc. Psychol.	53	6.1
J. consult. Psychol.	21	4.4

> The 233 pages of reviews in 1952 cost APA close to $6,000. The proposal is to take reviews out of these four journals and to consolidate them in one, with the $6,000 regarded as a saving to be invested in the new journal. The review journal is currently thought of as an economically produced affair going to all members of the Association. It is a fair estimate that the journal could be produced at a total annual cost of $18,000, and that a major proportion of this amount would be covered by nonmember subscriptions and by income from advertising.
>
> Some of the arguments for such a journal are as follows: The proposed new journal (a) would offer all members book and film reviews, not only in their own field of specialization, but also in adjacent fields in which they may not now see them, (b) could, through providing more space, keep book reviews more up-to-date than is now possible, (c) might permit especially good or controversial books to be reviewed in the same issue by more than one individual, (d) would prevent profitless duplication of reviews by two or more journals, (e) could produce book reviews in a less expensive and more expendable form than is now done, (f) would supply to librarians a desirable and efficient source of information about new books in psychology.
>
> Some arguments on the other side are as follows: The proposed new journal (a) would supply every APA member with reviews of books in fields in which he is not particularly interested, (b) would probably cost APA additional money, though additional costs could be at least partially offset by increased advertising, (c) might not achieve as much readership of reviews as would appearance of reviews in regular issues of existing journals, (d) would take up additional space on members' already crowded shelves, (e) would disturb an existing procedure that works well enough as it is.
>
> At the request of the Publications Board, the Central Office has presented this general proposition to a sample of our members with a request that they give their ideas about it. Guided by the results of this poll and by other considerations, the Publications Board may or may not recommend that the journal be started and the Council of Representatives may or may not approve a positive recommendation. Members who feel strongly one way or the other would help matters along by writing to Paul Farnsworth, chairman of the Publications Board. (*Amer. Psychol.*, 1954, **9**, pp. 39-40).

Somebody obviously instructed the Executive Secretary to write that piece. It was almost certainly the Board of Directors that did the actual instructing but very probably at the request of the Publications Board. A copy of the questionnaire mentioned here and a tabulation of the results can probably be found in the deep dark APA cellar, but there are no published data and probably no official record of how many people wrote to Paul Farnsworth about the idea.

The May 1954 issue of the *American Psychologist* (Vol. 9, p. 221) carries a notice by the Executive Secretary that the Publications Board would meet at the end of May, and, among, other

things "discuss the desirability of the Proposed APA Book Review Journal." The use of capitals here may symbolize a firming of the idea. The Board of Directors, meeting earlier in the spring, had probably discussed it or else the Executive Secretary, who remembers himself as a neutral and obedient servant, would not have mentioned it.

In June of 1954, the *American Psychologist*, very probably with the encouragement of both Boards, carried an article by Edward Girden and Wayne Dennis (Vol. 9, pp. 251-253) that dealt with general problems of book reviewing and with the specific idea of *A* Book Review Journal. This article reports that the poll mentioned by the Executive Secretary in January showed "considerable enthusiasm for the idea" of a new journal of book reviews and then went on to present, with more precision, some facts that showed a) that psychological journals with some frequency failed to review significant books (e.g., neither Penfield and Rasmussen's *The Cerebral Cortex of Man* nor Philip Vernon's *The Structure of Human Abilities*, both published in 1950, had been reviewed by the end of 1953 in any psychological journal searched) and b) that, in general, there was considerable time between publication of the book and the appearance of a review (with the exception of the *Journal of Consulting Psychology*, which published short and prompt reviews, and the *Psychological Bulletin*, a period of two years or more between publication and review was not at all unusual.) These observers also found what they regarded as profitless duplication of reviews in the various journals. These findings, plus some financial considerations, led Girden and Dennis to cite the following advantages of an APA book review journal:

> a. A central responsibility and a consistent policy for book reviewing.
>
> b. A more complete coverage of psychological books.
>
> c. Possibly an increase in the promptness with which book reviews are published.
>
> d. Greater convenience for readers in having all reviews in the same journal. (*Amer. Psychol.*, 1954, 9, p. 253)

The proceedings of the 1954 annual meeting of APA carry the brief entry: "It was voted that Council approve in principle the suggestion that APA establish a Book Review Journal, to review books, films, and tests, publication to commence in 1956, if possible."

It began to look certain, then, in September, 1954, that APA would actually produce an offspring. But not all APA members were enthusiastic about the prospect. Erwin K. Taylor, for one. He sent a strong letter to the *American Psychologist* criticizing the Girden and Dennis article for not studying all the important psychological journals and he made some points that must surely have influenced the deliberations of the Publications Board and that still can evoke williwaws of anxiety in a book review editor. Among other things Taylor said:

> a. Central responsibility and a consistent policy for book reviewing would impose upon the reviews the particular biases and emphases of a single individual whose particular specialized interests could not do other than bias his emphasis.
>
> b. Provide less adequate coverage of psychological books since no single editor could be a master of all specialties and would not be as likely as the present editors or the proposed specialized book review editors to be fully aware of the publications in his areas of specialization.
>
> c. Possibly *decrease* the promptness with which book reviews are published. A large part of the delay in the publication of book reviews stems from the difficulty in finding a competent reviewer willing to perform the chore and then prodding him as necessary to complete it. The specialized book review editor with his greater familiarity of interests of the personnel in his field would, it seems, be more successful in predicting which of his colleagues would be interested enough in a book to review it. (*Amer. Psychol.*, 1955, **10**, pp. 131-132)

The Publications Board and Board of Directors both apparently worked in their spring meetings in 1955 toward the actual parturition of the baby that the Council of Representatives had recognized to be there. Later records show that there was agreement on E. G. Boring as the obvious and perhaps only possible man to serve as first editor, and apparently Boring had accepted. The next published word on the enterprise appears in a note by the Executive Secretary in the July 1955 *American Psychologist*, again obviously at the behest of the Board of Directors (and again with obedience but with some slippage of neutrality). The Executive Secretary wrote:

> For the first time since 1946, when it launched the *American Psychologist*, APA is starting a new journal. The journal will be devoted primarily to review of psychological books but, in the thinking of the Publications Board and Board of Directors, it should be structured and chartered so that it may include, as the *Zeitgeist* changes and new needs evolve, other kinds of content. Its title was a final distillation from the attempt to clothe the journal in something more semantically revealing than a tent and less inhibiting than a strait-jacket.
>
> APA members have talked for two years about starting a new journal for book reviews. The idea originated in the Publications Board. The Board poked the idea around for an hour or two at one meeting, expressed a general interest and asked the Central Office to collect some facts, financial and otherwise. There was something appealing in the notion that psychologists could find in one place a critical and a relatively current assessment of all psychological books. And many liked the idea of presenting to outsiders—to libraries, to colleagues in other disciplines, and perhaps to interested laymen—a convenient exposure to our new books.
>
> At a second meeting, furnished with some basis for estimating income and expense, the PB found itself still liking the proposed venture. A poll of a sample of APA members revealed much approval and little dissent. A concrete proposition was put to the Council of Representatives in September 1954. The council voted yea and told the PB to get about the business of creating the journal; to secure, through regular processes, a good editor and to whip the Central Office into doing the necessary administrative chores. An editor—E. G. Boring—was

elected by Council during the Spring of 1955 and by May, PB, with the approval of the Board of Directors, had set down general lines within which the new journal would attempt to live. It will contain an annual 384 pages, including advertising, will appear monthly, will contain reviews of books and of films, will, with Council approval, be sold to outside subscribers for $8 a year and to APA members for $4, and will be a whomping success, both scientifically and financially. (*Amer. Psychol.*, 1955, **10**, pp. 308)

That piece was extended and significantly improved by quoting E. G. Boring's views—views later expanded and published in various issues of *CP*—of the functions of the journal, of the role of the editor, and of procedures and mechanisms for ensuring that the journal live up to what Boring termed its "pediatric prognosis."

The November, 1955, *American Psychologist* carried two items announcing the official birth of *CP*.

> 4. Approval of the submission to Council, with Board endorsement, of the name of Edwin G. Boring as a slate of one for election as Editor of the book review journal. (*Amer. Psychol.*, 1955, **10**, pp. 714)

> 2. The Board voted to recommend that the name of the new book review journal be *Contemporary Psychology: A Journal of Reviews;* that it appear twelve times a year at monthly intervals; that it begin publication in January, 1956; that it not be distributed automatically to all APA members, but that subscriptions be on a voluntary basis; that the subscription price to nonmembers be $8.00 per year; that the page allotment for 1956 be 384 pages, including covers and advertisements; that the journal contain reviews of films, but not reviews of tests; and that a 1955 budget up to $1,500 for editorial expenses be approved. (*Amer. Psychol.*, 1955, **10**, pp. 719)

The first issue of the journal appeared in January, 1956. As far as it and its friends can tell, E. G. Boring is the only real father it ever had, for certainly it was he who gave it its character and personality.

Books to Come

Soon to come or perhaps by now in the book stores is Eugene Gendlin's *Experiencing and the Creation of Meaning* (Free Press of Glencoe). Dr. Gendlin, who works with Carl Rogers at Wisconsin, says that his book, stemming from his own studies in philosophy and his own experience as a psychotherapist, "attempts to stand *between* the undefined richness of experiencing (undefined as for example, the 'existential' orientation views it) and the precisions of the definitions of logical positivism. It claims that neither should be altered but the area between them should be worked out systematically."

H. J. Eysenck is editing two books from his own department, both to be published by Pergamon Press. The first, due in the fall of 1962, is *Experiments with Drugs*. The aim of this book, Dr. Eysenck says, "is to test my drug postulate which says in effect that depressant drugs have extroverting effects while stimulant drugs have introverting effects." The work is in line with Dr. Eysenck's belief in the fruitfulness of the experimental manipulation of personality variables. The second book, the manuscript of which will be completed soon is *Experiments in Motivation*. This volume will report extensive research on drive. Recently *CP* wrote to Dr. Eysenck requesting a photograph to accompany the review of "his recent book." The photograph came, along with the polite and interested enquiry "which book?" The book was *Handbook of Experimental Psychology*, (CP, April, 1962, **7**, 126-128).

Elizabeth Duffy's *Activation and Behavior* will be brought out by Wiley in the late summer or early fall. The book has grown out of Dr. Duffy's longtime research concern with energy level, degree of excitation and energy mobilization. The book will focus on energy mobilization, rechristened "activation," treating this concept as a crucial and unifying one for many behavioral phenomena. *CP* gets the impression that Dr. Duffy was activated, or at least triggered, to produce the book by the fact that she ran out of reprints to send to those who requested them.

Charlotte Buhler's book *Values in Psychotherapy* (Free Press of Glencoe) will appear this summer. Dr. Buhler's aim, she reports, is to help the psychotherapist clarify his role in the patient's search for new values in life. The flavor of the book perhaps can be inferred from the following statement from Dr. Buhler: "Our parents and educators, it seems to me, do not give us very valid information regarding the ultimate reasons for pursuing the values they recommend. Thus many of us end up in psychotherapy and with psychotherapists who up to now have also not concerned themselves systematically with the values they stand for." The volume will contain a review and systematization of the relevant literature and will contain also concrete suggestions for further research.

Dehospitalization

Louis Linn (Ed.)

Frontiers in General Hospital Psychiatry. New York: International Universities Press, 1961. Pp. xxvi + 483. $10.00.

Reviewed by Stanley Blumberg

Louis Linn, who edited this volume, is a psychoanalytic psychiatrist now associated with the Mount Sinai Hospital in New York and with the College of Physicians and Surgeons of Columbia University. The reviewer, Stanley Blumberg, is a clinical psychologist educated at Northwestern University and trained in the Veterans Administration. He now is an instructor in psychiatry at the University of Texas Southwestern Medical School in Dallas and also serves as clinical psychologist at the Woodlawn Psychiatric Hospital.

This book, dedicated to the World Federation for Mental Health in honor of World Mental Health Year (1960), is a collection of 36 separate reports of current psychiatric practices within general medical and surgical institutions. Nineteen of the 36 chapters describe these services in foreign countries, including two African nations, the Virgin Islands, Japan, and Canada, as well as most of the European nations.

The most common theme running through these separate accounts is that there is a definite trend away from the custodial care of psychiatric patients in large, over-crowded, socially isolated mental hospitals and toward active, much shorter-term treatment within a general medical setting close to the patient's home. The virtues of this trend are extolled by most of the contributors, typically chiefs of psychiatric services with only a few dissenters, and the reader is soon imbued with a new, more optimistic hope for the future of psychiatric treatment. Perhaps more important than the hope which this book will give workers of all disciplines in the mental health field is the genuine excitement they are bound to feel when presented with new ideas which have been put into actual practice. For example, there is the apparently successful experience Dr. Main has had in treating hospitalized mothers of young children in England by admitting them only on the condition that their children be admitted to the hospital with them. Such a practice adds a new dimension to the concept of the hospital community as a treatment center. Similarly, there is the experience of a hospital in Quebec which admits the relative, usually the spouse, along with the patient in certain selected cases. Here is a challenge to the long-held notion that treatment is facilitated by removing the patient from his stressful home environment.

Other accounts of the successful treatment of psychiatric patients on mixed medical and surgical wards, in Day Hospital programs, Night Hospitals, and Home Care situations, as well as the more usual Out-Patient clinic are most stimulating. Even geriatric patients, the organic brain-damaged, and chronically psychotic patients in an acutely disturbed state are being successfully managed without long-term commitment to mental hospitals.

Although as yet, there are too few general hospitals in the United States with their own psychiatric treatment services—and even fewer in several European countries (e.g., Italy, Germany, Switzerland)—the evidence is mounting that where such services have been instituted beneficial results ensue; not only has individual patient care been improved, the community better covered, and basic attitudes toward psychiatry and mental illness altered, but the overcrowded conditions of the large mental hospitals have been relieved. While offering no final solutions, *Frontiers* points some ways to the future.

Diagramming Dysfunctions

John Bucklew

Paradigms for Psychopathology: A Contribution to Case History Analysis. Philadelphia: J. B. Lippincott, 1960. Pp. xii + 236. $5.00.

Reviewed by Edwin S. Shneidman

The author, Dr. Bucklew, is Associate Professor of Psychology at Lawrence College in Appleton, Wisconsin, where he has been these last fourteen years. The review tells more about him. He contributed several sections to N. H. Pronko and J. W. Bowles' Empirical Foundations of Psychology *(Rinehart, 1952). The reviewer, Dr. Shneidman, is Associate Clinical Professor of Psychiatry (Psychology) at the University of Southern California, where he is Co-Project Director of the Suicide Prevention Center and Co-Principal Investigator of the Veterans Administration Central Research Unit. Right now he is on leave at Harvard University working on the psychology of suicide with Henry A. Murray.*

—E. G. B.

Practically everybody has heard the apocryphal ancient Chinese proverb which states that one picture is worth a thousand words. That this precise ratio of illustration to text was meant as a mandate for books in psychology seems improbable, but Professor John Bucklew is, in the present instance, a modern disciple for this point of view. His first sentence tells us that "the writing of this book was undertaken because the author believes that visual models are as effective as words or numbers for integrating knowledge." He continues: "The volume summarizes several years of intensive work with case-history material in psychopathology devoted mainly to bringing some semblance of order out of the welter of recorded facts, interpretations, and special terminology which characterize the case-history method. In the course of his work the author gradually developed a conceptual and visual framework useful for analyzing case histories, which seemed to make more comprehensible to himself and students the intricacies of abnormality." Dr. Bucklew then tells us something of the development of his notions: "Starting originally with an attempt to visualize the dynamic relationships employed in a single case history, the analysis finally progressed to the form of paradigms which stand generally for ranges of abnormality. The next step in the development of the work required defining and describing each element in the diagrams by means of theoretical constructs drawn from abnormal and general psychology . . . The final step was to relate the constructs which had emerged to some system of psychology."

This, then, is a book which attempts to illustrate the case-history method. The book itself contains nineteen rather abbreviated, verbally described case histories which are visually illustrated in the Bucklew manner. The cases are drawn, as the author states, from those already available in the literature of psychopathology and are taken from such well known source books as those by Burton and Harris, Maslow and Mittelman, Alexander and French, Cameron and Magaret, and Weinberg and Hire.

Dr. Bucklew is Associate Professor of Psychology at Lawrence College in Appleton, Wisconsin. He received his

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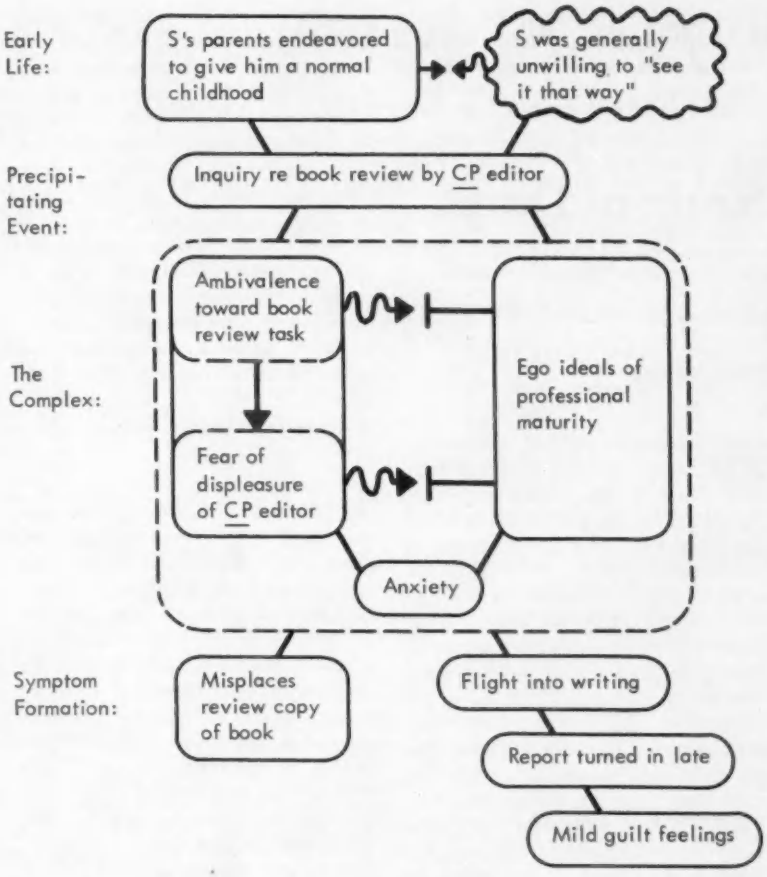
Early Life:
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Ego ideals of professional maturity
Anxiety
Symptom Formation:
Misplaces review copy of book
Flight into writing
Report turned in late
Mild guilt feelings

Bucklew's Code Applied to a Book Reviewer's Crisis

PhD from Indiana University and has taught previously at Oberlin College in Ohio. He has a general interest in the psychology of language. The author describes himself, within this book, as an "academic psychologist whose investigations in psychopathology have sprung from his interest in the extension of systematic psychology to the field of personality functions. Although his first-hand experience with abnormality is minimal, having been restricted mainly to wartime duties, work with student adjustment problems, and contact with local mental hospitals, he claims a compensatory advantage of being uncommitted to any one theory or program in psychopathology."

Dr. Bucklew's system is built around several basic concepts which he uses in diagramming case histories. These concepts, and their diagram elements, are: ego role and ego-alien motives (rectangles); motivational conflict (arrows pointing at one another); repression (a wavy lined arrow blocked by a line); regression (arrows pointing up); fixation (lines pointing down); and other concepts such as the complex (which is a conflict between ego and ego-alien motives); symptom formation; anxiety; specific life events (which are precipitating, traumatic, or conditioning); external conflict; developmental sequences. The bulk of the book (pp. 72-187) is taken up with the verbal and visual schematization of neuroses, inadequate personalities, psychopathic personalities, complexes, dreams, and psychotic reactions. The method of visual schematization can be illustrated by the following relatively innocuous fictional case:

> Case 20, Clinical Psychologist, age, 43, married, four children; he 'misplaced' the review copy of this book and, as a consequence, turned in his review to the editor of *CP* one week late, and then experienced mild anxiety and guilt feelings. This case is illustrated in the accompanying figure.

Professor Buckley indicates his intellectual debt to Gordon Allport, to Sherif and Cantril, Ausubel, Sarbin, R. W. White and especially to J. F. Brown. Bucklew's diagrams are, in some ways, modeled after Brown's use of nonmetricized topological space representations of personality structure. There is little question in the reviewer's mind as to the captivating and instructive nature of Bucklew's diagrams, but there is some question as to whether his concepts represent a substantial improvement over Brown's (and Lewin's) concepts of membership character, freedom of locomotion, permeability of boundaries, etc. Since Bucklew is a student of Brown's, he should also be aware of some of the shortcomings of class-theoretical thinking. Although Bucklew would seem, appropriately, to imply that each personality would have its own visual representation, he nonetheless labels his chapters with such class titles as "The Paradigm for Inadequate Personality." Further, in his interesting, and all too brief chapter on "The Case History as a Social Document," (8 pages) he speaks of "the" case history, implying a point of view which stands in contrast to the clinical notion that many individuals are capable of producing a series of successive anamneses. However, this mote of methodological myopia in Bucklew's view does not detract from the very substantial teaching potentialities of his visual schemes. It might, however, be accurate to think of his book not as containing specific paradigms for psychopathologies but rather as presenting illustrative illustrations of anamneses. Thus, the virtues of this book are most visible in the visual schematizations which fill it.

The book presents to the teacher or supervisor in clinical or abnormal psychology a useful pedogical device. Clin-

ical supervisors might well require their trainees to construct topological diagrams for the cases which they study. I would believe that Dr. Bucklew's book would find a welcome place (albeit a supplementary place) in many a clinical and abnormal course or practicum in psychology.

Many-Faceted Study of Drugs

Leonard Uhr and James G. Miller (Eds.)

Drugs and Behavior. New York: John Wiley, 1960. Pp. xix + 676.

Reviewed by J. R. Wittenborn

The editors of the present volume both work at the University of Michigan Mental Health Research Institute, Uhr as a research associate and Miller as director. Uhr has done research on the behavioral effects of a number of the new drugs and has reviewed for CP *one book on assorted new drugs* (CP, *Oct. 1959,* **4,** *330) and another on alcohol, an old and familiar one.* (CP, *Jan. 1959,* **4,** *21f). James G. Miller, who conceived and organized the Mental Health Research Institute and moved it from Chicago to Michigan, holds both the PhD and the MD degree from Harvard, where, on the non-medical side of the Charles River, he was associated with both Boring and Murray. He is now writing a book on living systems, to be published within the next year or so. The reviewer, J. Richard Wittenborn, is a University Professor at Rutgers University. An Illinois PhD, he spent a number of years at Yale as a research oriented clinical psychologist before moving, in 1954, to Rutgers. He conceives of himself as a biologically oriented psychologist and, as such, has done research on affective depressions and on drive. But he also has studied adopted children and personality development.*

The editors have brought together material of interest to a diversified group of clinicians and investigators. As a consequence, their combined contribution to the behavioral study of drugs is most comprehensive and is offered here in the form of 51 separate reports of research and clinical considerations.

Such a varied array of contributions rarely contains uniform quality, and the present collection is no exception. Chapter 1 by Roger Russell and Chapter 51 by Leonard Uhr are among the contributions which would meet the criteria of the most critically discriminating reader. Numerous other chapters scattered throughout the volume are incisive, pertinent, and authoritative. Among them would be included the chapter by Cole, "Behavioral Toxicity," the chapter by Vandenberg, "Behavioral Methods for Assessing Neuroses and Psychoses," and the chapter by Freyhan, "Psychopharmacology and the Controversial Clinician." There are other chapters equally rewarding in their excellence.

There are some chapters, however, which can best be described as unaccountable. There is a chapter, "The Design and Conduct of Experiments," by Harvey Nash, which neglects to describe the use of analysis of covariance in handling data gathered by the important and time honored pretreatment-posttreatment testing regimen. Masserman's chapter, "Drug Effects on Experimental Neuroses in Animals," comprises about three pages with approximately one-fourth of the material explicitly devoted to the production of experimental neuroses, but includes no mention of the classical work of such investigators as Pavlov or Liddell. Reference to the literature is not neglected, however; his list includes 26 separate entries, each bearing the author's own name. Fleishman's chapter presents a similarly myotic view of psychomotor behavior, a rich and traditional area of psychological inquiry.

Among the chapters are some which have a value that is neither methodological nor substantive, but which can broaden the reader's awareness and help maintain the investigator's orientation with respect to the total context in which his efforts must eventually be viewed. Chester Bennett has described his own subjective experiences in response to experimental dosages of various drugs, and Lawrence Kubie reminds us of the adjustive, motivational characteristics of man as they relate to the ultimate validity of pharmacological attempts to influence behavior.

If the reader is interested in the general topic of drugs and behavior, this volume will not suffice. It is a specialized contribution developed around contemporary interest in drugs as they are currently used in the control of behavior disorders. Nevertheless, Uhr and Miller have provided a useful source book for the clinician or investigator with contemporary interests. Regardless of the reader's purposes, it is probable that he will find several useful chapters which may broaden his view either from the standpoint of methodological approaches or from the standpoint of substantial contributions by other investigators. Fortunately, the content is not exclusively behavioral in its orientation, and Chapter 2 by Himwich and Chapter 3 by Berger make explicit some of the most distinctive known chemical characteristics of the various drugs and presumably the most relevant metabolic and structural characteristics of the human organism.

Although the multidimensional and interdisciplinary approach to the study of drugs and behavior does not find explicit recognition, the diversity of professional talents and methodological approaches is at the very least prophetic and may indeed inform us that from the standpoint of behavioral investigations, at least, we are now in an era where it is no longer sufficient for the investigator to organize his total efforts about the classical model of one dependent variable and very few, or only one, independent variables.

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Designed to supplement Sartain, North, Strange, Chapman: *PSYCHOLOGY: Understanding Human Behavior, Second Edition*, this collection of readings will help students increase their knowledge and interest in understanding human behavior from the psychological point of view. The articles selected illustrate how principles derived from scientific and clinical observations can be applied to the solution of everyday problems. Each chapter contains readings on the psychological aspects of the process under discussion; the important biological correlates of the psychological process; and socio-cultural determinants.

☐ **READINGS IN INDUSTRIAL AND BUSINESS PSYCHOLOGY, *Second Edition***

By HARRY W. KARN and B. von HALLER GILMER, both at Carnegie Institute of Technology. Available in June.

This collection of original papers represents current thought and research in the area of industrial and business psychology. Over 800 articles were examined before a final decision was made. The new edition contains more articles of an experimental and theoretical nature and for the first time includes selected readings in the areas of organizational behavior, engineering psychology, communications and the psychology of perception.

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STUDY II. Empirical Substructure and Relations With Other Sciences.

VOLUME IV. Biologically Oriented Fields: Their Place in Psychology and in Biological Science.

Edited by SIGMUND KOCH, Duke University. 752 pages, $12.50.

The fourth volume in a vast inquiry into the status and tendency of psychological science. Study II seeks an increased understanding of the internal structure of psychological science and its place in the matrix of scientific activity. This volume deals with the special biologically oriented fields.

☐ **STATISTICAL PRINCIPLES IN EXPERIMENTAL DESIGN**

By B. J. WINER, Purdue University. McGraw-Hill Series in Psychology. Just published.

This text provides statisticians and experimental psychologists with basic principles used in the construction of experimental designs. Examined are designs found in current experimental literature and those with unique and potentially useful features. Their advantages and disadvantages are studied in detail. Examples are drawn from areas of experimental, industrial and clinical psychology.

☐ **INDIVIDUAL IN SOCIETY: A Textbook of Social Psychology**

By DAVID KRECH, RICHARD S. CRUTCHFIELD and EGERTON L. BALLACHEY, all at the University of California, Berkeley. Just published.

The purpose of this book is to organize fact and theories from general psychology, personality psychology, social psychology, anthropology, and sociology, showing the interrelationships which exist among these areas. The four main study areas are: basic psychological factors; attitudes; social and cultural habitat of man; group and organizational behavior. A complete revision of Krech and Crutchfield's *Theory and Problems of Social Psychology.*

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The chapters are organized under the following topics: Biochemistry, Physiology, and Pharmacology; Methodology; Clinical Considerations; Experiments on Animals of Potential Application to Human Subjects; Objective Assessment of Normal Human Behavior; Techniques for Assessing Autonomic, Motivational, Pathological States; Experimental Use of Observational Techniques; and Controlled Subjective Measures. The reader need not be particularly interested in neuropsychopharmacology in order to find substantial value in this book.

The list of contributors includes many distinguished investigators and leaders in clinical thought, and the editors are to be congratulated. The Prologue by Uhr and Miller contains wisdom for both the experimenter and the clinician and should not be overlooked.

A Sharpened Managerial Tool

William McGehee and Paul W. Thayer

Training in Business and Industry. New York: John Wiley, 1961. Pp. x + 305. $7.50.

Reviewed by HARRY W. KARN

Both authors are psychologists, both have worked in academic settings and both have moved out into business and industry. William McGehee is Director of Personnel Research and Training at Fieldcrest Mills in North Carolina. Paul W. Thayer is Assistant Director of Research for the Life Insurance Agency Management Association. The reviewer, Harry W. Karn, is Professor of Psychology at Carnegie Institute of Technology. After receiving his PhD at Clark University he went to the University of Pittsburgh, then to war, then, after three years as a personnel and training officer in the Navy, to Carnegie Tech. In addition to basic research in learning and perception he has studied and written about training and has had extensive experience as a consultant on management development and on safety training programs.

ANYONE concerned with the effective utilization of human resources in the industrial organization should welcome this book with open arms. McGehee and Thayer have presented a closely-knit treatment of concepts and principles that sensibly and soundly describe and explain the operation of an effective training program — an acknowledged "must" for achieving organizational goals in a business enterprise.

Despite the plethora of writing on the topic of training—including articles in professional and trade journals, training manuals and chapters in textbooks—there has been a notable absence of systematic presentations that bring together into a single account the pertinent theoretical and factual material on training that is scattered throughout the literature. The authors of the present book have undertaken this task with striking success.

The book is framed by an introductory overview of the critical training problems that have emerged from practices now being followed in industrial concerns. Management people charged with training responsibilities will cringe at the description of the near chaotic state of most industrial training programs and at the conclusion that "training is probably the least effectively used management tool in industry and business." But there is a way out of the woods and this can be discovered by securing reliable data as a basis for answering the following questions:

Who is to be trained?
In what are they to be trained?
By whom are they to be trained?
How are they to be trained?
How are the results of the training to be evaluated?

MOST of the volume is directed toward suggested approaches to securing answers to these persistent industrial training problems.

Three chapters are devoted to a treatment of the problem of training needs. The authors develop a tri-fold rationale consisting of: (1) organization analysis—determining where within the organization training emphasis can and should be placed; (2) operation analysis—determining what should be the contents of training in terms of what an employee must do to perform a task, job, or assignment in an effective way; (3) man analysis—determining what skills, knowledge, or attitudes an employee must develop if he is to perform the tasks which constitute his job in the organization.

A critical examination of the literature within the rationale and framework of training needs enables the discerning reader to pinpoint the strengths and weaknesses of already available techniques for determining training needs and to see the gaps where research is needed. Such an approach, and appropriate future action, should be substituted for the "present standard equipment of training directors—hunches and the crystal ball."

This reviewer confesses that he has found it difficult to be critical of the book as a whole, because of his enthusiasm for two central chapters having to do with learning theory and the principles of learning as they apply to industrial training practices. The reviewer claims familiarity with many industrial training programs and he is appalled at the apparent lack of recognition that the psychology of training is actually the applied psychology of learning. The conclusion is inescapable when one sees established learning principles being ignored or violated in so many industrial training programs. Of course, the central process in training is learning and McGehee and Thayer argue convincingly that, despite gaps in our knowledge, enough is known about learning to serve as a basis for the institution of sound industrial training programs.

This reviewer suspects that the failure to utilize established learning principles in industry and business is partly due to the inaccessibility of the relevant source material. This excuse may have had some validity. After all, one cannot expect industrial training personnel to keep abreast of research and theorizing as reported in professional books and

journals. But the dilemma no longer exists, for the present volume presents a frame of reference for thinking about learning principles and theory that serves as a meaningful guide for directing industrial training activities. This framework provides the basis for assessing the various methods and techniques now used in industrial training.

The book closes with a cogent treatment of the problem of industrial training evaluation. The authors admit that this is not an easy problem since it demands a knowledge of statistical techniques and methods of controlled research. Cost-conscious management men who usually accept training as imperative are reminded, however, that only by proper evaluation can full value be received from training dollars.

This may not be the best book that can be written on training in business in industry, but it will serve nicely until a better one appears.

Procrustean Engineering

Paul A. Verdier

Basic Human Factors for Engineers: The Task Analysis Approach to the Human Engineering of Men and Machines. New York: Exposition Press, 1960. Pp. 103. $4.00.

Reviewed by HAROLD P. VAN COTT

who is manager of The 4732 Personnel Subsystem Group of the Federal Systems Division of the IBM Corporation. Since taking his doctorate in experimental psychology at the University of North Carolina, he has worked in and on systems at the USN Medical Research Laboratory at the American Institute for Research and most recently at IBM. He has had a hand in research on BMEWS, SAGE, and on a number of equally significant but less well known systems.

SCARCELY a score of years has passed since the birth of a new branch of applied psychology variously called *Human Factors, Human Engineering,* and *Engineering Psychology.* This field has used the background and principles of the life sciences in the design of a span of systems from the kitchen stove to behemoths such as SAGE and Mercury. Psychologists, like F. V. Taylor, J. C. R. Licklider, and Paul M. Fitts have made scholarly and useful inputs to its annals.

The experimental data of engineering psychology cover the gamut from training and maintenance to personnel selection and instrument design. But in methodology, the field has a conspicuous gap: there is no concise way systematically to predict, describe, and analyze what man will do in a system, how he will do it, what skills will be required, and what problems must be solved to achieve an efficient, safe, economical man-machine system.

A book that would present such a method sets a high goal for itself; a number have tried. *Basic Human Factors for Engineering* has such a goal. The means to the goal is contained in the book's subtitle. Neither the name nor the method is new, having been described and used by J. M. Christensen in 1949 and R. B. Miller in 1953. Like its predecessors, Verdier's task analysis achieves some success at the expense of cumbersomeness. But this new treatment has additional disadvantages: notably, oversimplification and unguarded enthusiasm.

The book starts with a preamble on the benefits to be realized by industry if it uses task analysis. We are told that task analysis is a practical method that will clarify the role of the human engineer, enhance his status, and simplify his duties. Work will be less difficult, the environment may become less hazardous and more comfortable, and mental health benefits may accrue to the worker on his job, in his social life, and at home. In the future, we read, task analysis may help create a situation in which "All that will manifest itself will be an insidious increase in quality, production, wages, and profits." Even more enticing is the statement: ". . . . task analysis provides the method whereby modern engineering design principles can be resolved by the logical application of its principles to experience or empirical research with their time and cost restrictions." *Mirabile dictu!* And all in 103 pages.

THE book takes the "task" as a basic building block of the method. It achieves an affinity to Titchener's mental elements. Human factors are identified from the human demands made by tasks. These factors, we are told, are of four types: perception, judgment, internal stress, and motor ability. Each is defined: internal stress ". . . . is quite an illusive factor which, while it cannot be discounted, does not occur very frequently." Like other task analysis methods, use is made of preprepared formats for systematizing the task analysis data. And, like H. Bamford's method, use is made of special symbols; however, no instructions are given for insuring a valid basis for establishing whether or not a given task does contain a particular human factor or what its impact may be on task performance.

The basic task data may be expanded into a flow-chart. Time-line methods may also be used to assist in scheduling operations. Link-analysis techniques are reviewed as a means for achieving an effective layout of equipment and people.

Regretfully, the book falls short of its goals in novelty, depth, scientific validity, and practical utility for the engineer. To the engineer, task analysis as it is described may yield a wealth of data but fails to provide, by itself, and without the assistance of a skilled specialist, the analytic and research skill needed to interpret and apply the data. To the life scientist, the book is a scientific disappointment. While claiming "sound psychometric methodology," we fail to find any genuine evidence of it.

The restive reader may turn to the discussion of the human engineering department or to the treatment of a national human factors society in the hope of finding new or useful material. We are told that a human engineering department will provide ". . . . interesting fringe benefits for the company (because) the human engineering

method advocated in this book will eliminate the very cause of much emotional disturbance in workers, and thereby save consultant's fees." On a human factors society, which the book believes should be established, one cannot help but wonder whether or not the Human Factors Society (founded in 1956) and APA's Division 21 on Engineering Psychology have failed in publicizing their existence.

There is a legend that Procrustes was the first human engineer. An ancient Greek robber, he engineered his victims to fit an iron bed by stretching the short ones to its length and cutting the tall ones down to size. Perhaps if *Basic Human Factors for Engineers* had been stretched out in places and cut down in others, it could make the job of the engineer-life scientist team an easier one.

What is an Observation?

G. R. Pascal and W. O. Jenkins

Systematic Observation of Gross Human Behavior. New York: Grune & Stratton, 1961. Pp. x + 126.

Reviewed by JOHN A. STERN

Both authors, Gerald R. Pascal and William O. Jenkins, are now at the University of Tennessee, Pascal as director of the Psychological Service and Research Center and Jenkins as director of the Psychological Laboratories. John A. Stern, the reviewer, has been at Washington University since receiving his PhD at Illinois in 1953. He is now head of the Division of Medical Psychology in the School of Medicine and director of the Behavior Research Laboratories. He has studied the behavior, both at a gross and at a physiological level, of two and four footed organisms. He reports an increasing allergy to rats accompanied by a growing preoccupation with human behavior, including a special pedagogical involvement with students of clinical psychology and of medicine.

THIS book addresses itself to advanced undergraduate and graduate students "interested in human behavior as it occurs in life situations." It attempts to give the student a theoretical position and methodology for exploring human behavior. Its strength, the presentation of one and only one position, is at the same time its principal weakness. One may question whether one should, as this rather brief volume does, present to the inquiring student, one theoretical position to the exclusion of all others. One of the major assets of this book lies in the detailed exposition of the classes of behavior to be observed, the stimulus categories and the responses to be recorded. The development of the observational scales is systematic and logical in approach and has generated some interesting findings as described in a later section of the book.

Though the book is entitled *Systematic Observation of Gross Human Behavior*, only some 30 or so pages are devoted to the exposition of this approach. Had the title been based on the remainder of the book it might better have been entitled *Systematic Recording of Self-Observations of Gross Human Behavior*. This is what the major portion of the book deals with. According to the authors the chief instrument to be used in this investigation is the "fallible observer." After reading the book it appears to this reviewer that there is more fallibility in the recommended procedures than the authors are willing to state. To wit, if the procedures detailed in the book are used as described the fallible data we end up with are based on the fallible observer (recorder) recording the fallible reports of the subject's fallible self-observations of selected aspects of his behavior. It would seem that there are too many intervening variables, too many fallibilities here to achieve the avowed purpose of the writers—to encourage the development of a science of human behavior. Other approaches to this lofty goal are briefly mentioned but disposed of rather summarily.

The approach of the writers is S-R in character, with the organism specifically excluded. The stimulus situation is inferred from the subject's response. Incorporated into the stimulus situation are the subject's past experiences. (If one can't have an organism the stimulus seems to be as good a place as any in which to relegate past experience!) Although the response is the major variable to be investigated the authors seem to have considerable difficulty in deciding on what constitutes a response. At one point of their presentation they end up defining a response in terms of whether it elicits a response in another person. This is a rather tight and circular operational definition of what constitutes a response and one rather difficult if not impossible to apply! Thus, if the stimulus has to be inferred from the response, and what is to be considered a response is in some doubt, it seems that a science of behavior built on this foundation is liable to collapse before the venture gets well under way.

THE book's approach to the study of behavior is avowedly behavioristic. The major concern expressed is for a study of man as he responds to "stimuli in his natural environment," the most important of which is considered to be other people. Some restrictions are made as to what may be a stimulus. Early in the book the content of the subject's verbalizations are not considered legitimate stimuli while the non-content or non-verbal aspects of his verbal behavior such as rate and intensity of speech are acceptable data. In the final analysis we unfortunately find that the elicitation of reports about the behavior of the subject by the subject rather than the direct observation of

the subject constitutes the data on which a science of human behavior is to be based. Thus content, as long as it does not concern itself with unobservable behavior such as feelings, hopes, and attitudes is the data we end up with. Why reports of feelings, etc., are attributed to the organism and thus excluded while other experiences are freely attributed to the stimulus is not explained although one is left with the feeling that feelings, hopes, attitudes, and pain associated with ingrown toenails are not fit subjects for a science of human behavior. Though they state that paper and pencil tests and "responses to questionnaires become for us merely paper marking responses to a stimulus situation which consists of paper, pencil, examiner, etc" (p 23), it seems that the data obtained by their approach is but one step removed from questionnaires and paper and pencil tests. Instead of feeding the subject questions through visual means and obtaining a response mediated through the use of a pencil by the respondent these authors substitute an oral presentation, an oral report, and the pencil is wielded by the questioner. Admittedly, this may lead to somewhat greater flexibility in the investigative procedure but there is little evidence that it leads to more valid, or even more reliable information.

In the final analysis one must decide whether a science of human behavior can be based on observations as limited as those described in this book. These authors are willing to make a case for this approach. Their procedure for obtaining their data are well described and the approach deserves a full trial. This reviewer has some qualms about developing a science of human behavior utilizing only one technique. It is our pessimistic conviction that what a person says and what he does are far from perfectly correlated, and that what he says he did is even more poorly correlated with what in fact he did in the past. It would seem to us that a science of human behavior, however gross, must have a broader base of observation than suggested by this book.

I would recommend this book for use in a course devoted to the development of techniques for assessing human behavior. It should however, be placed amongst a series of books or articles describing different approaches to this admittedly complex problem. By itself, it is rather meager fare.

Disarming Some Fuses

Gerald Caplan (Ed.)

Prevention of Mental Disorders in Children: Initial Explorations. New York: Basic Books, 1961. Pp. 425. $8.50.

Reviewed by C. L. Winder

Gerald Caplan, the editor of the present volume, is a psychiatrist who is Associate Professor of Mental Health at the Harvard School of Public Health and Director of the Harvard Community Mental Health Program. C. L. Winder is professor of psychology and Director of the Psychological Clinic at Michigan State University. For more than a decade he has been involved in the training of clinical psychologists and in research on schizophrenia, personality development and psychotherapy. He believes in eclecticism and in the fruitfulness of diversity.

This collection by twenty-one contributors emerged from a conference organized by the International Association for Child Psychiatry and Allied Professions. The problem addressed is primary prevention, which "was defined at the Hague Conference as the promotion of mental health and the lowering of the risk of mental disorder in a population of children by interfering with pathogenic forces within the children and in their biologic, psychologic, and social environment before the appearance of identifiable pathology" (p. 17).

Contributions are classified by the editor as follows: four papers on organic factors, four on family process, five on consequences of crises, and three on school settings. The implied scope of concepts and information, which serious workers in prevention must attempt to grasp, threatens to become overwhelming. There is sufficient firm content throughout this range to give substance to the familiar assertion that mental health must be approached on a multidisciplinary basis.

Acceptance of the content of this volume as a valid gauge of extent of knowledge about prevention leads to the conviction that wide reaches of almost unknown territory lie in every direction. There is much more that the contributors can say about what they are studying than about results in hand.

In several reports, there is illustration of the intricate, subtle interplay of substantive with methodological contributions. One sees that exploitation of a new concept or point of view occurs in proportion to inventiveness in the arena of methodology. Refinements of new concepts frequently follow as results are obtained with new or revised methodologies.

Some contributors to this book show appreciation of the fact that relatively specific interventions may give rise to ramifications which would not always be anticipated. For example (and Tarjan's contribution is explicit on this point) any program resulting in decreased time span between generations might result in enhanced effects of both desirable and undesirable genetic variables. Ojeman's concept of "social causality matrix" implies that modification of the teacher-role might result in unfavorable response by substantial segments of the tax-paying population. There is a clear implication in statements by contributors Pasamanick and Knoblach that an improved survival rate among very premature infants may increase the number of mental health problems due to risk of brain damage and maladjustment. And Caplan says that if there were a reduction in the frequency of occurrence of interpersonal incidents which are traumatic for the majority, then the few individuals who would have learned to cope with the problem situation may be less capable of meeting other stressful events.

In general, interventions will often have side consequences and some of these may be distinctly undesirable.

Many individuals working in the mental health professions have been duly skeptical, in this reviewer's opinion, of so-called general theories based on delimited laboratory experiments. One must reflect, also, that there is need for recognition of the fact that mental health field studies may yield results specific to particular types of situations.

Although the contributions to this volume are heterogeneous, there is particular emphasis on the management of crises as an especially promising focus for preventive programs. Crises are to be managed so as to prevent disorganization of individuals and groups, and so as to reinforce effective problem solving. The search for procedures which implement this strategy has been intensive. Many ingenious and challenging variations on the major theme are reported.

Warmed over Borscht

Ralph B. Winn, Hans Hiebsch, A. A. Smirnov, A. N. Leontiev, N. F. Posnanski, G. S. Kostiuk, and A. L. Shnirman

Soviet Psychology: A Symposium. New York: Philosophical Library, 1961. Pp. 109. $3.75.

Reviewed by DANIEL E. BERLYNE

Ralph B. Winn, who edited the translation from German of the present volume and who wrote a foreword to it, is Chairman of the Department of Psychology at Monmouth College. Daniel E. Berlyne, the reviewer, very recently became Associate Professor of Psychology at the University of Toronto, moving to that position from Boston University. He was educated at Cambridge University and at Yale, has taught at the Universities of Aberdeen and St. Andrews and at Brooklyn College. In 1961, with a grant from NIMH, he visited a number of research institutes and laboratories in Russia. His 1960 book, Conflict Arousal and Curiosity, *was reviewed in* CP *in May, 1961.*

THE publication of this book in this form is a great mistake. And now that it has been published, it would be a great mistake for anybody to read it in the hope of learning what contemporary Soviet psychology is like.

The book consists of brief articles by five Soviet psychologists, including Smirnov, Leontiev, and Kostiuk, three of the outstanding figures in present-day Soviet psychology, together with a foreword by Ralph B. Winn and an introduction by Hans Hiebsch.

The collection is subtitled "A Symposium," but there is nowhere any indication of the origin of the material—where and in what circumstances the symposium was held, and when. Such a practice would seem to be unfortunate at any time, but in this instance it is particularly regrettable. Internal evidence scattered through the text leads one to conclude that the material dates from quite some time ago. This conclusion is corroborated by a sentence from Hiebsch's introduction: "The victory of T. D. Lysenko's biology in August, 1948, marks the end of the second stage and the beginning of the third, in which Soviet psychology *now* finds itself" (the reviewer's italics).

This book thus presents samples of Soviet psychology from the period of which Lysenko was the baleful personification. Now, anybody who has been following developments in the USSR, however perfunctorily, knows that this period is over and that Soviet science, including psychology, has undergone profound changes since the death of Stalin. In June, 1954, the biological journal of the Academy of Sciences of the USSR denounced Lysenko's genetic theory as "essentially unsubstantiated and mistaken" and, in some instances, "clearly falsified." Lysenko resigned from the Academy in 1956. He has since been re-admitted, but as an expert on agriculture rather than as a pundit on genetics. Soviet scientists are still not completely free from the pressures of ideological orthodoxy, but, at any rate, the grotesque obscurantism that marked Lysenko's heyday is now a thing of the past.

How far things have changed in psychology depends on what area one examines and how near it comes to treading on ideological corns. In the "physiology of higher nervous activity," which includes much that we should regard as physiological psychology or as learning theory, there is now virtually no iron curtain. Russian and Western physiologists have been visiting each other, joining each other in symposia and conferences, collaborating on research, and freely borrowing each other's research techniques and ideas. Men like Anokhin and Beritov, who were severely castigated in 1950 for their departures from Pavlov's experimental methods and their original extensions of his theories, are now in positions of great influence and respect, being rightly celebrated both inside and outside the Soviet Union for these same creative innovations.

Soviet psychologists, as distinct from physiologists doing what we should call psychology, are still at pains to associate their work with a Marxist-Leninist orientation and to repudiate the misguidedness of Western 'bourgeois' psychology, with its susceptibility to the errors of 'idealism' and 'mechanism.' But they do this without the animosity that formerly marked their references to the intellectual products of the West and, in fact, with no more rancour, and sometimes less, than one Western psychologist will show towards another with whom he disagrees. In any case, they no longer hesitate to refer freely and with approval to work done in the West, or to point out and develop the close relations that often link their findings, interests, techniques, and theories with ours.

It is, admittedly, a good thing for Western psychologists to acquaint themselves with what Soviet psychology was like during the last years of Stalin's life, not as an encouragement for self-righteousness, but as an object lesson in what can happen when the aims of science are subordinated to extra-scientific influences. Extra-scientific influences assumed disastrous proportions in the Soviet Union during that period, but such influences are, of course, not confined to totalitarian states. Nor are they always governmental in origin.

The articles collected in this book do not, however, even give a good picture of that time. They are mostly concerned with problems of developmental and pedagogical psychology, as is most of what goes by the name of psychology in the USSR, but they are replete with proclamations in favor of Lysenko's genetics and objurgations of those who oppose it. They are too brief to say very much, and they represent the kind of discursive, polemical discussion of generalities that has always formed a large part of Soviet psychology but not the part that most urgently warrants our attention.

There is, at present, a great hunger among Western psychologists for news of what their Soviet opposite numbers have been doing. It is saddening to see the fare in this book served up for the assuagement of this hunger. The book can only be grossly misleading. There is nowhere any indication that the Soviet psychology it depicts is that of a bygone era, and there are passages like the one quoted above from Hiebsch that encourage the contrary belief.

Psychoanalysis Still on the Couch

Jules H. Masserman (Ed.)

Science and Psychoanalysis. Vol. IV: Psychoanalysis and Social Process. New York: Grune and Stratton, 1961, Pp. xii + 196. $6.75.

Reviewed by PAUL E. BAER

The editor, Dr. Masserman, Professor of Neurology and Psychiatry, Northwestern Medical School, author of Practice of Dynamic Psychiatry *(Saunders 1955) and* Principles of Dynamic Psychiatry *(Saunders 1946, 2nd ed. 1961) among others, and recipient of the Lasker Award in Mental Hygiene 1946, should be well known to* CP *readers. The reviewer, Dr. Baer, a Chicago PhD, is Assistant Professor of Psychology at Baylor University College of Medicine and Houston State Psychiatric Institute, and teaches part time at the University of Houston. His interest in psychoanalysis was formalized by participation in the Psychoanalytic Child Care Program of the Chicago Institute of Psychoanalysis. It has since been modified he reports by several years at State University of Iowa College of Medicine and a growing involvement with psychophysiology.*

SINCE reviews of the three earlier volumes in this series were entitled "Psychoanalysis Still Unanalyzed" (*CP* Sept. 1959, **4**, p 273f); "Whither Psychoanalysis" (*CP* July 1960, **5**, 223f); and "Psychoanalysis on the Couch" (*CP*, May 1961, **6**, 158f), this review is most appropriately titled "Psychoanalysis Still on the Couch." The patient is progressing, thank you, although slowly, and the treatment may be interminable. In this case, the progress indicated so far deserves whatever reinforcement will accelerate improvement and keep alive the process of change. For the change is toward active participation of psychoanalysis in the scientific market place, and the trend is developmental. It is misleading, however, to speak of psychoanalysis as if it were a single entity, or as if there were conceptual consensus concerning it. The history of the psychoanalytic movement has been singularly punctuated by factional differences. These have mostly been of a charismatic type, in which splinter groups have attached themselves to intellectual leaders, who were innovators or dissidents, such as Adler, Jung, Rank, Horney, Fromm, Sullivan, Rado. The six-year-old Academy of Psychoanalysis, to which the papers in the present volume owe their existence, is distinct from the American Psychoanalytic Association, and represents a movement based not on charisma, or cult, but on shared interest and common goals. It is thus open to more critical investigation and healthy dissension, and may not reflect the main stream of psychoanalytic thought.

Many readers who share an interest in the current status of liberal psychoanalytic thought, but whose areas of specialization differ greatly, will find stimulation in this psychoanalytic sampler. This small volume of papers presented at a meeting of the Academy in May, 1960, cannot be treated as if it were organically whole. Using the term social process in its broadly inclusive title, the volume is diverse and variegated, and might more appropriately have been labeled proceedings. One finds in it research on the process and outcome of psychotherapy, a report on technique in the classical psychoanalytic case study method, discussion of psychoanalysis in political science, anthropology and sociology, a conceptually thin culture-personality study of Mexico, an exposition of family group psychotherapy, a review of problems in psychoanalytic training, and a statement of psychotherapeutic process—a statement, incidentally, of matchless clarity and simplicity. There is a prefacing presidential statement reflecting renewed interest by psychoanalysis in its own growth and its relations with other fields of knowledge.

Contributions include papers by well known workers, such as Parsons, Lasswell, J. Frank, N. Ackerman, and Aldrich. The papers are given trenchant discussions by Kardiner, Bullard, J. Spiegel, G. Ham, Grinker and Appel. Interesting reading for those who may have to discuss the role of psychoanalysis in psychology and wish to know what not to do is LaBarre's barren

bibliographic review on psychoanalysis in anthropology with its masterful criticism by Kardiner, (apparently an annual performance.)

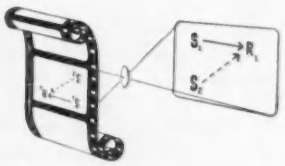
MUCH of what is offered here is provocative in that it is indicative of the new look in psychoanalysis. The need to test hypotheses is acknowledged, with examples of research, and there is explicit recognition of the trap of religionistic revelation. Psychologists whose scientific rigor is offended by the stereotype of psychoanalytic dogma may be pleased to note references to conditioning, predictive probability and imprinting by several contributors as mechanisms useful in understanding psychotherapy and personality development. Nevertheless, this calls for cessation of doctrinal isolation in psychoanalytic training and the opening of the psychoanalytic consultation for study and observation, are matched by reports of research lacking adequate controls, measures and hypotheses, and by an inclination on the part of analysts to feel that social sciences use psychoanalytic ideas with insufficient sophistication. The flow of psychoanalytic concepts to social sciences often serves only to fill in gaps. But concept transfer by psychoanalysis and social science has left marks on each. Psychoanalysts, as suggested in this volume, now appreciate the influence of social institutions in psychopathology, and regard psychotherapy as a social process. The prevalent view of the effectiveness of psychotherapy discriminates between an affective-relationship-need for help dimension and a cognitive-insight-attitude change dimension. Psychologists interested in research on psychotherapy are advised to read Frank's contribution. Still unanswered questions for the reviewer are: just how can psychoanalysis relate to social science, and can psychoanalysis be regarded as a social science? Some beginning answers might be found in the kind of work done by Miller and Dollard and in the development of social learning theory. Unfortunately, potential links between psychoanalysis and social psychology are ignored and there is no discussion of topics such as self theory and social perception.

At the broadest level of interpretation, the volume speaks for greater contact between disciplines, for a desire for scientific respectability, and presents some candid and entertaining self appraisal by a profession which is working to maintain survival value against the challenge of changing and shifting public and professional pressures.

On the whole, the caliber of contributions is high and much of the material worthy of attention. A special dispensation to the teacher who is struggling with the beginning student's refraction to psychoanalytic concepts is Silverberg's paper "An experimental theory of the process of psychoanalytic therapy" a masterpiece of explanation.

INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



Plato, Pigeons and Programs

Learning and Behavior—(The Teaching Machine). (CBS News Conquest)

Carousel Films, Inc., 1501 Broadway, New York 36, N.Y. 16 mm. sound, black and white, 26 min. Sale: $135. Rental: $15 per day.

Reviewed by LAWRENCE M. STOLUROW

The reviewer is Professor of Psychology at the University of Illinois, where he has also for some years headed the Training Research Laboratory. He got his PhD at the University of Pittsburgh, where he also taught from 1946 to 1951 before going to Illinois. At Illinois, he has conducted numerous studies on learning and problem solving for the Air Force, from 1951 to '57; on the teaching of exceptional children, from '57 to '59; and since that time on programed instruction. One of the latest of his many publications is a 173-page monograph, Teaching by Machine, *published by the U. S. Office of Education.*

SKINNER, Collingwood, Psychology and CBS combined talents and forces to produce an informative and impressive film. Whether used in the college classroom or in adult education meetings this film will provide a useful springboard for discussions of such topics of current interest as the control of behavior, reinforcement, programed instruction, and the application of psychological techniques to the management of learning both individual and social.

The basic process of operant conditioning is clearly communicated and its general implications and potential relevance to a wide variety of animal and human behavior is discussed. Plato's dialogues developed the Socratic method to its highest point and remain the classic and unexcelled example of effective and skillfu! instructional programing. The dialogues do not play a direct part in the story told by Skinner and Collingwood, but indirectly the relationship is suggested. Behavior of pigeons and people can be shaped by careful control of its consequences. In the dialogues the carefully worded question leads to responses that are the basis for further questioning, and in the tight interaction of give and take there gradually emerges a new preception and possibly a new understanding of an intellectual problem. This also is the paradigm of the operant conditioning procedures as they are

demonstrated in the gradual modification of the pigeon's behavior as it relates to both animate and inanimate objects. The potential potency of reinforcement as a mechanism in the shaping of behavior is clear and obvious. Skinner's scalpel in shaping the pigeon's behavior is selective reinforcement of its "spontaneous" segments as they appear and conform to a desired pattern. We see that through this single mechanism the possibility exists for an active rather than a passive psychology of learning.

Students, viewing both pigeon and Peggy learning the same discriminations within a programed sequence of patterns, can't help but be impressed with the relevance of basic research on learning to the management and hopefully, the understanding of socially significant performance. One might wonder, however, just what the film does, in fact, convey to different groups of viewers.

To answer this, the film was shown to selected groups of viewers who might provide useful data. Afterwards an attitudinal questionnaire was administered to determine the effect it had upon them. An information questionnaire was also used with the student groups. The two attitude questionnaires were based upon those developed by Greenhill (1955). One contained 17 items and was administered to Psychology 100 students at Illinois (a general introductory course with 208 students, mainly sophmores and juniors) and to a group of education students in a course in Educational Psychology ($N = 30$, mainly seniors). The latter group were mostly in training to be teachers. The second questionnaire was given to three groups of judges: (a) eight graduate students in psychology; (b) seven graduate students in speech, and (c) nine professional psychologists (many of whom were working as school psychologists).

All groups saw the film and then completed the questionnaire immediately afterwards. Florence Tromater assisted in collecting and analyzing the data; Barbara Morgan, Leone Smith, and Professor Walter McAllister cooperated in having the film shown to their classes and permitting the collection of data on the student's reactions.

The groups of undergraduates showed some similarities and some interesting differences in their ratings. They used the scale differently. The education group tended to rate all items higher (range of means was 3.16 to 4.71) than did the psychology students (range from 1.71 to 4.58). If we simply look at the ranks of these ratings we see a significant difference in the order of the 26 items. In general the lowest ratings occurred on the last 9 items which were added to the 17 item set used by Greenhill.

The tendency was for all students to rate the film at the high end of the scale. Over 60% of the items were, on the average, rated above 4.00 by the psychology students and over 88% of the items were rated over 4.00 by the education students. Both groups agreed (top rating) that the information presented in the commentary was well integrated with that presented in the pictures. The education students rated two other items almost as high—that the film attracted and held their interest, and that the method of presentation was suitable to the subject matter. The psychology students also included the latter in their first three ratings but, as their second point, said that the film presented a learnable amount of information (other extreme: attempted to present too much material to be learned at one time).

In the opinion of the psychology students, the film did not build upon previous knowledge and the education students agreed by rating this at the low end of the list. However, while at the low end of the list of items, the rating still was above 3.5 (on the average) for both groups.

Some interesting reactions were found with the 9 items we added to determine the students' reaction to content related issues—teaching machines, control of behavior, science of behavior, psychological research. (The differences that existed despite seeing the film are of interest even though there is no way to show from these data the extent to which similar differences may have existed prior to seeing it.)

Of these 9 items the groups differed significantly ($p < .05$) in their mean rating on the following four items, in which the scale goes from 1 to 5, from left to right:

			Students	
			Psych.	**Educ.**
18.	An objective science of behavior, determinism, is something			
	which can be developed in time	which can never be developed	2.34	4.20
23.	Behavior is controlled by			
	external factors	the person himself	2.43	4.20
24.	Results from psychological research will			
	allow man to understand himself and thus is desired.	allow people to control others, and thus is undesirable	1.71	4.36
26.	Teaching machines have been said to be a threat to teachers, for they might be replaced by them. What is your opinion about their replacing teachers?			
	This is all wrong.	This is inevitable.	2.17	3.90

Ten factual multiple-choice questions were analyzed to determine what the students knew after watching the film. The mean score for 200 undergraduate students was 5.23. By chance we would expect 2.00 correct. The difference is significant, indicating that they did know something about the factual content by the end of the film (either from having seen it or from prior experience).

None of the groups of experts differed from the others to a significant degree, although neither the average values nor the ranks agreed for all three groups except on one item: Did the film provide for adequate repetition of the important content? All groups rated

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ROBERT T. GOLEMBIEWSKI is assistant professor, Department of Management, University of Illinois, Urbana.

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this as their lowest item, but the lowest average was 3.44 on a 6-point scale. At the other extreme, two groups agreed in giving the highest average rating to this item: Did the film attempt to present too much material to be learned at one time? The general concensus was that it did not. In no comparison was the mean significantly different for any pair of groups.

By both experts and students this film was well received, considered to be of good technical quality, effectively presented and organized.

Don'ts in Testing

Intelligence Testing of Tom. Educational Film Library, Syracuse University. 16 mm. sound, black and white, 52 min. Sale: $250. Rental: $8.75 (1 to 5 days).

Reviewed by BENSON H. MARSTEN

The reviewer is Clinical Supervisor for the Department of Psychology's Clinic School at the University of California, Los Angeles, where he also has taught extension courses in elementary psychology. Since receiving his PhD in clinical psychology from UCLA, he has specialized in diagnostic psychological testing, parent counseling and the training of graduate students in these fields.

THE purposes of this film, as stated in the introduction, are three-fold: (1) to present an actual intelligence testing situation to graduate students in psychology; (2) to reveal test behavior of "an unusually gifted boy" of 12 years; and (3) to contrast this boy's performance on the Wechsler Intelligence Scale for Children (WISC) with the results of a previous Stanford-Binet. (This boy had also been tested previously with a WISC but the test protocol was lost.) How well does this film meet these objectives? In this reviewer's opinion, not at all well.

Regarding the first objective, this film might be used to teach students what *not* to do in administering an intelligence test. Careless technique was evident in *E*'s leaving the open manual in easy view of the subject except when *E* became aware of this and quickly covered the manual with his arms. But this *faux pas* is minor compared to *E's* commitment of the cardinal sin of objective testing: viz. significant deviation from standard administration. Not only are instructions often not given verbatim, and sample items not correctly presented but, perhaps more important, *E* at times would not accept the *S*'s erroneous responses and would press for another response by asking leading or pointed questions. Wechsler is very explicit and insistent on these matters. These rigid requirements for adherence to standard administration of objective tests are not, of course, peculiar to the Wechsler scales. Apparently *E* was over-anxious to demonstrate this child's unusual ability. On the positive side, *E* did seem to have good rapport and showed a pleasant, friendly and generally reassuring attitude to *S*.

The second objective, demonstrating gifted test behavior, doesn't come off well. While this was certainly a very superior youngster, much of Tom's test performance could not be described as "unusually gifted." *E* explains this at the close of the testing as due, in part, to their "nervousness" in front of the camera. While this is probably true, it does not satisfy the viewer's or student's curiosity and interest in unusually gifted test performance. Incidentally, the cinematography is unvaried during the administration of the Verbal scale but the camera does move in for some good close-ups of behavior on the Performance scale.

The third objective, contrasting Tom's WISC with previous Stanford-Binet performances is adequate but rather general and quite brief. A more detailed comparison of WISC and S-B performances might have been more instructive although this might have made the film too lengthy.

A film of this kind could be valuable in training students in test administration if the major deficiencies outlined in this review were rectified. It seems worth mentioning that the film as is may have instructional value for triggering discussion of do's and don'ts of test administration.

ON THE OTHER HAND

HAIL PENNSYLVANIA!

The moving finger writes and, having writ, moves on—sometimes to correct its own errors; but how rarely the same reader reads both! *(CP,* Dec. 1961, **6,** 430.) Eugene Galanter did take his PhD in 1953 at the University of Pennsylvania, but not, as *CP* reported, under Nelson Goodwin, with whom he did a lot of work, but under William A. Shaw, a comment from him that starts one wondering whether in this context *under* has a specific meaning. At some universities it does not. Galanter also wants the world to be reminded that the Department of Psychology was organized at the University of Pennsylvania in 1887, when (let me add) Cattell, fresh from Wundt and Galton, was appointed Lecturer there. (See J. McK. Cattell, *Amer. J. Psychol.,* 1890, **3,** 281-283). The Penn lab may even have been the third in the USA. Ruckmick, in his list of the first 37, gives in order the [unorganized, unfounded] Harvard lab (1875), the [organized and founded] Hopkins lab (1888), Indiana, (1889), Wisconsin (also 1883), leaving out Penn (C. A. Ruckmick, *Amer. J. Psychol.,* 1912, **23,** 517-531, esp. 520). *CP* had said that the Department at Penn was "newly organized" in 1958, whereas the fact is that it was newly *re*organized in 1958, seventy-one years after it was first organized. It was from Penn that Fullerton and Cattell's rebellion against Fechnerian psychophysics issued in 1892.

EDWIN G. BORING
Harvard University

(For the record, CP *has made another slip in identifying Robert R. Bush as chairman of the "newly organized" department at Pennsylvania* [CP, *March, 1962, 7, 86.] In March, just as in December, "newly" should be "re-.")*

WHICH DREVER?

Belatedly I have just been reading the September number of *Contemporary Psychology.* In it I found myself—*Parochial but Wise*—credited with a book which at first I did not even recognize. Then I remembered. Shortly after the Second World War I was approached by a firm of publishers who produce, in accordance with an admirable nineteenth century fashion, a sort of do-it-yourself culture kit. A volume of collected papers in each subject is accompanied by a short explanatory booklet. I was asked to re-write the booklet for Psychology, and to suggest one or two additional papers for the volume. I had forgotten all about this.

The book reviewed with such kindly perplexity last September was, I think, compiled by William Brown, who succeeded McDougall at Oxford as Wilde Reader.

It is a real period piece, and not a modern reproduction. Even the Drever article is by my father.

I should be very grateful if you could let this be known. Other friends beside your reviewer will feel puzzled at what they take to be my choice. Much more important is that credit should be given where it is due.

JAMES DREVER
University of Edinburgh

BOOKS RECEIVED

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Psychological Monographs: General and Applied. Longer researches and laboratory studies which appear as units. Published at irregular intervals during the calendar year—between 15 and 20 issues per year. The 1962 volume is Vol. 76. Subscription: $10.00 (Foreign $10.50). Single copy $1.00 to $3.00 according to size.

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